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PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Remarks on the Diagnosis of Abdominal Swellings, in Relation, chiefly, to Dilatation of the Colon.*—By HENRY KENNEDY, A.B., M.B., one of the Physicians to Sir P. Dun's, and the Cork-street Fever Hospitals.

SOME time before the death of the late Sir Benjamin Brodie he published a letter against quackery. It was marked by the strong common sense and great sagacity to be found in all his writings. Amongst other points he took occasion to advert to the necessity which exists for making our diagnosis of disease as accurate as possible; and, obvious, very obvious, as the point is, it may still be questioned whether it receives all that attention at our hands to which it is entitled. For myself, I believe that quackery is much more closely connected with legitimate medicine than many of us would at first be inclined to admit. I have very rarely known the public rush, in the first instance, to quackery, but only after legitimate medicine had failed; and I consider that every single step in advance, whether in diagnosis, or other direction, is, by so much, a lessening of the domain of quackery; and that the time may with reason be looked for when this domain will be reduced to a minimum.

As to its entire extinction this can scarcely be expected; for legitimate medicine cannot, I fear, be ever made so complete as to be placed amongst the exact sciences.

These thoughts have arisen from the nature of the subject to which I would at present ask attention, which relates mainly to the diagnosis of some forms of abdominal tumours—a subject of much interest, and no little difficulty. Of the three great cavities the diseases of none present more difficulty in diagnosis than those of the abdomen. Nor would I except from this statement even tumours, or other disease of the brain. The late Dr. Bright, in his masterly monograph on abdominal tumours, has alluded to these difficulties, and fairly, as I think, attributed them to the number of the organs in the cavity, as also to their possible displacement or mal-position.

But there are other difficulties besides these which must have been experienced by all. The contents of the hollow organs—air, feces, bile, or urine—frequently cause the diagnosis to be difficult. It might be supposed that the last of these, from the fixed position of the urinary bladder, could not lead to mistake; and yet, in the last volume of the *Transactions of the Pathological Society of London* will be found the details of a case by Dr. Murchison, in which a tumour of considerable size occupied the right inguinal region, pressing on the vessels so as to cause great œdema of the limb; and as the patient, a man of sixty-eight, seemed to be sinking, and the swelling, it was thought, might be an abscess, and so cause the danger, it was tapped, when a quantity of urine was drawn off. On *post mortem* examination, it was found that the tumour was caused by a large hernial sac, if I may so speak of it, which was formed on the side of the true bladder, with which organ the tumour had a distinct but narrow opening. It is to be observed that the patient had passed urine freely. There is a good drawing given of this remarkable case showing the site the tumour occupied. The difficulties, too, in making a diagnosis, may be much increased by the presence of ascites, as can be readily understood. Indeed I believe this to be the most frequent cause of any. And lastly, the state of the abdominal walls themselves may be, and often is, a source of difficulty. Though this may arise from œdema, which will not, of course, escape our notice, it more frequently arises from fatty deposit, which is generally easy enough to recognize. But it is not always so; and the following case is a good example in point.

In the course of last year a gentleman, of thirty-six years of age, applied to me, having become alarmed about himself on account of

some lumps, as he called them, which were growing in his abdomen. On examining him as he lay on his back, five or six very distinct tumours could be felt; they were of a uniform size, that of a middle-sized but flattened apple, and arranged from above downwards on either side of the umbilicus. At first I confess I was at a loss to account for their marked regularity, thinking I had some internal tumours to contend with. But closer examination showed me they were contained in the abdominal parietes; and the matter was finally set at rest by causing the gentleman to raise himself partially, when the action of the abdominal muscles, which in this individual were well developed, showed me at once that the tumours were between the skin and the muscles, and were, in truth, nothing but fat, which here had assumed the look of disease; owing, most likely, to the anatomical construction of the muscles, and their strong development, Though this case might appear to be a very common one, I cannot call to mind another like it; for, as a general rule, this is certainly not the way in which fat is deposited in the abdominal parietes.

Were it even possible, it is anything but my intention to enter generally into the subject of abdominal tumours. The question is one of great extent; and each organ requires—as has, indeed, been done in Dr. Bright's valuable memoir—a separate consideration for itself. So, on the present occasion, my remarks will be confined to a single affection giving rise to abdominal swelling, and one which seems to me not to have received all the attention it merits, even in the memoir to which allusion has been made. Before speaking of it, however, I would say a few words on a means of diagnosis which, when present, affords often a valuable aid; and I am not aware that any writer has noticed it: I mean the mobility of abdominal tumours as caused by the act of respiration. All are aware that some of these tumours can, by the pressure of the hand, be moved about. But it is the movement caused by inspiration of which I speak. The first occasion on which I noticed this occurred some years since, in the lifetime of the late Surgeon Neville. He had asked me to see a man who had a tumour in the epigastric region, of the nature of which he was doubtful. The question lay between a malignant tumour in the stomach and aneurism of the aorta. As the patient lay on his back, I thought I observed the tumour moving; and on closer scrutiny there could be no doubt of the matter, nor that it was due to the act of inspiration; and it at once struck me that it was a means by which a correct diagnosis might

be made; for it was obvious that if the tumour were an aneurism no movement of respiration could affect it; but if it were in the stomach this might occur. Acting on this, the diagnosis of a tumour of the stomach was made, and the result confirmed the truth of this idea, for, when the man died, some months later, malignant disease of the stomach was found. It is worth noting, that, between the time of my first seeing this man and his death, the tumour lessened materially; and this seemed due to the great and, possibly, rapid ulceration which had occurred in the diseased mass. Since this case came under my notice I have had many opportunities of confirming the point. It is quite obvious that its value is confined, in a measure, to the upper third of the abdomen, the depression of the diaphragm being its cause; and it can only affect an organ which admits of some mobility, as the stomach and transverse colon. But the opposite of this may also be of use to us to know; for a tumour not affected by the act of inspiration must then belong to one of the parts which are fixed, as, for instance, aneurism of the aorta. Since I observed this sign it has not fallen to my lot to meet a case of abscess of the liver coming forwards. But there is little doubt that in such a case, and with a swelling perceptible, the act of inspiration would cause the tumour to move; and that if adhesions formed between the tumour and the parietes this sign would then cease—a point of no little consequence to determine, as bearing directly on the treatment.[a] It may be observed, in passing, that the degree to which movement can be given to the parts close to the diaphragm varies considerably with the individual. Those who have the chest long, from above downwards, are able to depress the diaphragm, and necessarily the neighbouring organs, much more than others. This, indeed, is only what we should expect from the varying capacity of the chest in different persons. But it is curious to what an extent it exists; and in exact proportion with this will be the intensity of the sign to which attention is now called.

[a] Since writing this I have recalled to mind a case so remarkable as to be worth noting here. It will be found in the Medical Times and Gazette for 5th March, 1859, and was that of a man who presented a movable and markedly pulsating tumour in the epigastrium. This seems to have led to the idea that the tumour was literally a movable aneurism, and is so stated. On *post mortem* examination a mass of disease, which seems to have been strumous, was found in the liver; and to this, which had been observed to be moved by the act of inspiration, was communicated an impulse from an aneurism behind it; for this disease also existed in this remarkable case.

It is not to be understood, however, that the sign is entirely limited to the upper part of the abdomen. It may be well marked in the middle third, of which I have seen different examples. In one of these, a boy of fifteen, whom I had in Dun's Hospital for some time, a tumour, the size of a small apple, lay on the left side, between the floating ribs and the umbilicus, but nearer the latter. As the boy lay on his back, the tumour was quite visible, and could, of course, be felt; and, on making him inspire, it glided downwards one inch and a-half, and then rose again on expiration. Several gentlemen, including Drs. Law, Hudson, Moore, A. Smith, and Walshe, saw this boy; but, after the closest examination, no diagnosis approaching certainty was arrived at. The spleen was known to be enlarged; and it is a curious fact, and worth noting, that when the boy came into hospital the veins of the abdominal walls were much enlarged, but subsequently lessened so much as to be barely visible. My own impression of the nature of the tumour was, that it was a portion of indurated omentum, very probably the result of strumous disease. I had seen a similar case, some years before, with Dr. Cuthbertson, excepting that the tumour in this last case occupied the right side of the abdomen. Here it was only a *post mortem* examination cleared up the obscurity which hung over the diagnosis during life.

In reference to the distance to which the act of inspiration may affect abdominal tumours, I should say I saw one instance where a tumour which had reached the right iliac fossa was so moved. The case was one of malignant disease, which commenced above—I think in the liver—and grew with extraordinary rapidity till it reached low down on the right side. Here I could feel distinct movement when the patient made a deep inspiration. The explanation of this would, I think, be, that it was the movement of the liver above which was communicated downwards; and that, had the mass grown from below, the great probability is that no effort of the breathing would have affected it. At any rate, the fact that the respiration is capable of affecting movable tumours in the upper and middle thirds of the abdomen, and may even reach lower than this, is, I think, worthy of notice in reference to diseases of the ovaries, where it may be the means of arriving at a diagnosis as to the presence or not of adhesions, which, I should suppose, it would, at times, be of great consequence to know.

The affection to which I would now ask attention is dilatation of the colon—not the simple, and what may be called transient, state,

and which it may be supposed exists, more or less, in all cases of constipation, but that state of the bowel in which the dilatation is permanent, and from which it is very doubtful whether recovery ever takes place. The affection is obviously one analogous to what occurs in the other hollow viscera, as the heart, stomach, gall, and urinary bladders; and, though the natural and healthy functions of the colon might seem, as it were, to lead to dilatation, still this state is, in my experience, rare. Neither is its diagnosis as simple as might at first be thought. Indeed, mistakes, I know, may occur; other diseases may be mistaken for this one, or this state of the colon for others. An example or two may not be out of place:—

Case I.—A girl, of nine years of age, was admitted into hospital, labouring under considerable swelling of the abdomen. On examination a number of masses could be felt through the parietes. They were very movable, conveyed the idea of being so soft as to take the impression of the fingers, and were shaped just like coils of intestine. The general impression was, that the case was one of dilated bowel, filled with feces. It should be stated that the child had considerable fever. She wasted rapidly; and, as she did so, the tumours became the more marked, and were even visible to the eye. On *post mortem* examination a very considerable portion of the intestine, chiefly the smaller one, was found infiltrated with a lardaceous material, evidently of a malignant nature. It was so placed as to thicken the walls of the intestine, in some parts to the extent of half an inch, and thus lessen its calibre. The material was softish, and retained the impression of the finger. The case is, as far as my experience goes, unique.

Case II.—A maiden lady, middle aged, and of a spare habit of body, began to get large in the abdomen. It was so very gradual as to be almost imperceptible, and but little inconvenience was felt. There was a tendency to constipation, which, however, yielded to purgatives; but the size of the abdomen was not reduced after their action. The patient died, and the whole colon was found enormously dilated, and full of feces, the sigmoid flexure alone being four feet long and two feet in circumference. There was no stricture of any kind. This case has been given because the preparation has been preserved, and may be seen in our College of Surgeons; and, in the printed account of it, it is significantly added:—"The nature of the affection was not guessed at during life."

Case III.—It is now more than six years since I was asked to see a lady on account of swelling of the abdomen. She was unmarried, upwards of fifty years of age, and of a spare habit of body. The lower extremities pitted on pressure, and she could walk but feebly. Though occasionally in the habit of taking aperient pills, there was no marked constipation. The swelling came on so imperceptibly that she could not say when it first began. It was quite obvious it increased at the slowest possible rate. Her appetite was fair; but she was, and had been, subject at times to fits of low spirits. When seen in bed the abdomen was found to be much swollen, and the swelling gave the idea of great solidity; and percussion confirmed this, for there was very little tympany. The veins over the surface were visible, but there was no marked enlargement, neither was there any fluctuation.

My first impression of this case was that the tumour was ovarian. But closer examination led me to doubt this; for I found that the mass was made up of five or six smaller masses; and that some of these could be moved in a very slight degree, not, however, from the act of inspiration, for I believe the abdomen was too full to allow of this, but by pressure from the hand. The greater masses, I should say, lay on either side of the umbilicus, the longer axis being from above downwards. It is sufficient to add of this case, that after I had seen it three or four times, and having also in mind the case already given, of which the specimen is in the College of Surgeons, I came to the conclusion that it was one of dilated colon, and subsequent events have, I think, confirmed this opinion. For though the lady is, and, I suspect now, will be a confirmed invalid, she has yet been materially improved by treatment. The abdomen was considerably reduced; and as it became so, the tumours divided, if I may so speak, so that what was a single one this month, became two the next. She was able to take considerable walks, and otherwise could enjoy life. I only now see her at long intervals, as she is able to manage herself. But at the present time I believe she is as well as her disease can allow of. The treatment adopted consisted mainly in courses of tonic purgative pills, given for twelve or fifteen days at a time, and then stopped. No medicine in the fluid form either agreed with her, or indeed was of any use. Circumstances prevented my carrying out all the measures which might prove of use in a case of this nature. Her greatest inconvenience, I should state, does not arise from her size, but from the weak state of her limbs, and the inability of walking. She is worse, too, at

certain times than at others; and then there is usually considerable pitting of the lower extremities. From first to last there has been nothing of what might be called constipation, but more a sluggish action of the bowels.[a]

Within the last eight months another instance of this affection came under my notice. It occurred in a man of sixty-eight, who came to me under the idea that he was getting dropsical. An examination, however, satisfied me that the swelling, which was dull on percussion, and had no fluctuation, was due to dilated colon; and in about three weeks treatment relieved him, at least for the time. There is every probability, however, that the state will return—the more so when we recollect his time of life. The case, however, calls for no further notice here.

But all cases of this affection do not present themselves to our notice under the same uncomplicated form. Other states and diseases may be superadded; of which the following is a good example:—

Case V.—A married woman, about thirty years of age, was sent into Sir Patrick Dun's Hospital, by Dr. Leney, of Bray. She laboured under an affection of the heart, and had slight œdema of the lower extremities. She thought herself dropsical, even in the body; and when seen in bed the abdomen was found to be very full; but neither Dr. Aquila Smith, who saw this case, nor I, could detect fluctuation. The examination, however, did detect a number of tumours floating about, as it were, in the abdomen; for they were very movable. None were larger than an orange, and they ranged from that down to the size of a walnut. On inquiry, the patient stated that for upwards of a year the bowels had been getting irregular, but that otherwise she did not feel out of health. When, however, the pitting about the ankles came on, joined to the disease of the heart, as known to Dr. Leney, it was then thought

[a] After this paper was read, Dr. M. Eustace stated the following very apposite case:—A lady, aged twenty-eight, unmarried, got the habit of taking opium for pains in the abdomen. After some time the abdomen began to enlarge; and subsequently the lower limbs became weakened to such a degree that the patient was obliged to use crutches. In this state she came under the care of Dr. Eustace, who, from a very careful examination, concluded that no organic disease—for this had been suspected—existed in the abdomen, but that the swelling was due to fecal accumulation. Acting on this idea, she was treated by aperients, &c., whilst the use of opium was gradually discontinued. Under this plan the abdomen got smaller and smaller; and in the same proportion the lady recovered the use of her limbs, and was finally quite restored to health. She had been using the crutches for seven months.

advisable to send her in to Dublin. She was treated under the idea that the swelling of the abdomen was due to an enlarged and sacculated colon; the tumours being collections of feces, and there being also air in much greater quantity than is usual in this class of cases. At the end of three weeks all the movable tumours had disappeared, and the abdomen had lessened in proportion. Still there remained more of fulness in the hypogastric region than was natural; and though the patient denied all possibility of being pregnant—and this had been asked her on her first admission to hospital—it still turned out that she was so, as proved by the stethoscope. She ultimately left hospital as well as a woman in her state could, for the heart disease was unequivocal.

But cases may be very like the one just given, and yet not be identical. Shortly after this, my friend Dr. M'Clintock showed me a case where two or three tumours, the size of an apple, and very movable, could be felt in the abdomen. The patient was a woman of forty-two, and had been married but two years previously. The tumours lay on either side of the abdomen, and would be referred to the lower part of each kidney. Dr. M'Clintock's opinion was, that the tumours were malignant, whilst my own was that they were due to a sacculated colon; and under this idea she was sent to Sir P. Dun's Hospital, to see what a course of treatment might effect. She remained, however, but a fortnight, being anxious to return to her home. It is enough to say, that she did not get better whilst in hospital, and that, after many months, Dr. M'Clintock's opinion was proved to be correct, for she died of malignant disease, involving nearly the entire abdomen. Of this case I shall only add that, to the present time I am at a loss to say where the disease could have commenced. For I can scarcely imagine that the tumours first discovered were the ovaries. They seemed to me to be too movable, and at too great a distance from the usual sites of these bodies. This is, however, a matter for discussion.

There is one feature in the history of the affection of which I have been speaking which calls for notice here. As a general rule the bowels are sluggish, but not at all to the degree that we might expect. On the contrary, they may act regularly; and this is very likely, indeed, to lead us astray, and calls for the greater caution. So far, however, from there being constipation, or a tendency to it, there may be the very opposite, and diarrhea may exist. One such instance I have known; it was exhibited by the late Dr. Mayne before our Pathological Society. In this instance the diarrhea

terminated life, and the entire length of the colon was enormously distended. There was no ulceration, nor stricture of any kind.[a]

The diagnosis of dilated colon is not by any means the simple matter it might seem; and I believe it to be but common prudence to withhold our opinion till we are able to make successive observations on the case. Some, no doubt, are so very plain that the diagnosis may be made at once; but it may, I think, be stated with certainty, that the great majority are not so. In the case of the lady already given, she was seen, but once only, by the late Sir Henry Marsh, and he told me plainly that he could not make up his mind on the case; and in this I think he showed his great good sense.

On what, then, is the diagnosis to rest? I can only answer this in general terms. In the first place there are usually more tumours to be felt than is common where organic disease exists, whether malignant or otherwise. For though I have given a case where malignant disease, and in a comparatively early stage, existed in two distinct regions, it will still, I think, be allowed that this is exceptional, and therefore not generally to guide us. On the other hand, a single tumour may exist, and yet be due to nothing more than a portion of the colon dilated, and filled with feces. I have met no unequivocal case of this kind myself, but there are two—if I recollect right—in Bright's able monograph. Still, these are all exceptional cases. As regards the site of the tumours, in cases of dilated colon, something to assist our diagnosis may be gained. Thus they are commonly situated so as to occupy the middle third of the abdomen; and when they occupy, as they often do, both sides of the cavity, there are good grounds for supposing it is a dilated colon with which we have to deal.

But, probably, one of the most characteristic signs of this state is the way in which the tumours are impressed, as it were, with a

[a] Dr. Robert Smith has directed my attention to another case, exhibited by Professor Banks, and in which the termination was different from anything yet described. The patient, a man of fifty, was admitted labouring under sickness of stomach, and obstinate constipation. He had similar attacks before, but not so severe as the present one. There was very little constitutional disturbance at any time, and the abdomen, though much swollen and tympanitic, was not painful on pressure. More than two days elapsed before the constipation was overcome; but though the relief seemed to be complete at the moment, collapse and death followed on the third day from his admission. On examination, the disease was confined exclusively to the sigmoid flexure of the colon, which was of a greenish hue, and enormously distended—measuring two feet in circumference. There was no recent peritonitis, nor sign of stricture, or alteration in structure of any kind.

certain amount of movement—I mean in themselves. This can, of course, only be observed when we can make repeated observations at certain intervals. When this is done it will often be seen that the tumours have altered their position; slightly, it is true, but still so as to leave no doubt on the point. There is every reason to set this down to a very slow peristaltic action of the bowel. With it, too, will occasionally be observed a certain degree of lessening of some of the tumours; and, in the last place, it may occur that the character of the discharges from the bowels, as regards their size, may assist the diagnosis. It might be supposed that this would be of common occurrence. It is quite the reverse; for weeks and months may elapse and yet no characteristic discharges occur.

Such, then, are the signs which may lead us to a correct diagnosis:—The number of tumours—their site—their movements—their alteration in size—and, occasionally, the character of the evacuations.

Into the symptoms caused by this state of the colon it is not my intention to enter here, farther than to say they are, or may be, all those which arise from constipation in general, and, in addition, the increased size of the abdomen. The patients are very liable to attacks of pain like colic; and the two extremes of constipation and diarrhea, of which examples have been already given, may likewise occur. It seems scarcely necessary to add that our treatment must be pointless and ineffective if the state with which we have to contend be not thoroughly understood. Much caution, too, is required as regards our prognosis. With our present experience we have no grounds for supposing that the disease, if once fully formed, is curable. But that it can be alleviated is certain; and the extent to which this may succeed must depend on the circumstances of each case. The age of the patient, duration of the disease, length of intestine involved, temperament, &c., will all have an influence on the result. What, then, is the line of treatment to be adopted? Courses of purgative medicines, suited to the requirements of each case, given at stated intervals, and then omitted for the time, must be our sheet anchors. It is most important, too, that these medicines should be used with the least injury to the system at large. Hence the value of combining tonics with aperients; and of the former I have found none more efficacious than the preparations of the nux vomica. The common extract of this drug, in union with the compound colocynth pill, or the former with the mass lately introduced in the *British Pharmacopœia*—the

compound pill of colocynth with hyosciamus—make good combinations. Every one, however, will have his own favourites. The doses should not be large, but small, and frequently repeated—at least twice a day; and, except under some peculiar circumstances, pills are much superior to the fluid medicines. Injections, too, are at times useful; but it is very strange what little comparative effect they have.

The best directed medicines will, however, cause little good results unless they be combined with other means. The diet should be regulated so as to be the opposite of what is called a dry diet; and with this a system of frictions to the abdomen should be perseveringly carried out; and also bandaging, so as to give a steady support to the parts, should be enjoined. As a means of improving the tone of the muscular coat of the intestines electricity has been recommended, and in suitable cases would, no doubt, be of benefit; as would also all those measures which have a tendency to improve and invigorate the constitution. But these points are so obvious that they do not call for any further notice here.

Art. II.—*On the Induction of Premature Labour in the Sickness and Vomiting of Pregnancy, with Cases.* By S. L. Hardy, M.D., F.R.C.S.I., Physician, Accoucheur to, and Lecturer on Midwifery and Diseases of Women and Children, Steevens' Hospital; Physician to the Institution for Diseases of Children, Pitt-street, &c.

Vomiting, one of the most usual accompaniments of pregnancy, and considered so beneficial when within certain limits, occasionally becomes so violent, long-continued, and prostrating, as to endanger the safety of both mother and child.

Some cases of this nature yield to treatment—others, to a certain extent, are so far manageable as to enable the patient to complete her full term of utero-gestation—whilst a third class are so violent and uncontrollable as to require the induction of premature labour to rescue the unfortunate sufferer from a wretched and certain death.

Two cases lately came under my notice of very severe vomiting during pregnancy. One in private, the other in hospital practice. In the first premature labour was spontaneous; in the second it was induced. Both patients were saved.

Case I.—On the 14th of February last I was called to visit a lady, who expected her confinement about the 12th of March. She was extremely delicate—nearly the only member of her family who had not fallen a victim to consumption. She had given birth to several children, who were so feeble that but few survived. She had taken a carriage drive but two days previous to my seeing her, which was followed by violent spasms of the uterus. Several days passed before this attack subsided. Diarrhea and vomiting then set in. The former was amenable to treatment, but the latter was so violent and frequent that the strength rapidly gave way. Every means of treatment failed to afford more than temporary relief; and in addition to vomiting, the secretion of fetid gas in the stomach was most distressing.

The patient now became most desponding, and so reduced in strength and flesh, that great fears were entertained for her safety, and a question arose as to the course best to adopt in respect to her confinement.

On the 26th of February a consultation was arranged to be held, but at 4 o'clock, a.m., pains set in; labour proceeded without sickness of stomach or any untoward symptom, and at 5.25 a.m., a female child was born (seemingly about three weeks premature) The placenta came off a few minutes after, and the uterus remained well contracted; and when Dr. Churchill met me at 12 o'clock (by appointment, on the previous day) we found mother and child doing well.

From this until the 10th day the recovery progressed so favourably that the relatives of the patient, supposing all danger gone, informed her of some household arrangements, which had the effect of exciting her so that when I was sent for at 11, p.m., she was highly maniacal. The nurse-tender (a remarkably good one) she supposed was going to poison her. The pulse was very little above the natural standard, but her countenance depicted extreme terror and distress. Under the soothing influences of removing the nurse-tender from taking charge of her, and a large opiate, she had a good night's sleep, and was quite herself next day. Nothing whatever again occurred of maniacal symptoms throughout her convalescence; but there was extreme nervousness, which a look or word was sufficient to call forth into the greatest agitation—she then trembled violently, and became bathed in perspiration. This gradually subsided, and she was able to drive out, having got safely through all the dangers of her pregnancy and confinement.

However, the disease of which she seemed so likely to be the

victim at last began to develop itself. There was no cough, but a steady failing of strength, perspirations, loss of appetite, and occasional diarrhea.

Percussion over the upper part of the left lung gave a dull sound, and at length the symptoms of phthisis rapidly appeared, the lung becoming throughout affected until her death.

Case II.—In March last a woman, aged twenty-two, was admitted into Steevens' Hospital She stated herself between six and seven months gone in her first pregnancy. In the early months there was no nausea or sickness, but during the last month she had suffered extremely from violent vomiting, with constipation. The very emaciated appearance she presented caused doubts to be raised as to her condition; which were set at rest by the distinctly audible sounds of the fetal heart.

Burning in the epigastrium, with great thirst, were very distressing, and sickness, more or less troublesome, until the 30th of March, when, at 6 o'clock, a.m., dark fluid was ejected, with blood, in large quantity, and the burning sensation became intolerable. The fetal heart's sounds were weak and slow. Under the repeated administration of morphia and creosote some ease was procured, with a little rest in sleep. On the 31st her pulse, which on the previous day was extremely weak, had improved; but her condition was very uncomfortable, and she felt low and desponding. The fetal heart was inaudible, and the globular shape of the uterus could be distinctly perceived, by the hand, when passed over the abdomen. There had been no vomiting from 6 o'clock this morning.

I ruptured the membranes at 10.30, a.m., the head presenting. Uterine pains set in at 4.30 o'clock, p.m.; vomiting returned at 6, p.m., and so reduced the strength that it was necessary to deliver immediately, which Dr. Tynar, resident surgeon to the hospital, accomplished. The child, a female, appeared to have been a short time dead. After recovering from the shock of delivery, under the liberal use of brandy, opium, &c., she improved considerably during the night. Next morning some dark fluid was vomited, but not of the character of what had been previous to delivery. The stomach then gradually received and retained mild nourishment. On the third day there was diarrhea, which was easily restrained. Until the sixth day pain in the cardiac region, with a troublesome cough, distressed her. She then progressed

most favourably, and left hospital, recovered, on the 12th of April. Three weeks afterwards she presented herself, and had become so fat and improved in appearance that some did not, at first, recognize her.

Sickness of stomach, in the early months, was absent in both these cases; neither of the patients having complained of anything until far advanced in pregnancy. Dr. Ramsbottom remarks that when vomiting is entirely absent utero-gestation does not proceed with its usual regularity and activity. The vomiting of early pregnancy has been adduced, by Sir H. Marsh, as illustrating the nature of transmitted affections of the stomach. "No evidence of the transmission of disease" he says, "can be more convincing than that which is derived from the early stage of pregnancy. It illustrates, first, the true nature of transmitted disease; secondly, the either total or almost total absence of indications derivable from the real source and origin of the transmitted malady. That which strikes one as most wonderful is the fact that at an unexpected moment, the position and condition of the uterus being changed, all the suffering is terminated."[a] Those remarks, though made as to the changes which take place in the early months of pregnancy, may be applied also to the present case. The vomiting, which persisted notwithstanding every effort to restrain it, ceased immediately on the condition of the uterus being altered by delivery.

Irritability of stomach is observed in several affections of the uterus besides during pregnancy; the treatment being sometimes, by mistake, directed to the stomach instead of to the proper seat of the malady. This is known in the case of polypi. I lately removed a small polypus from the uterus of a patient who had been previously under treatment for eighteen months. Attention was altogether directed to the stomach as the seat of the malady. The removal of the polypus, which was effected by torsion, was followed, for two days, by aggravation of all the symptoms; they then entirely subsided.

Sir H. Marsh met with a case of nausea and vomiting so severe that life was despaired of. "The countenance was ghastly, the prostration death-like; disease of the uterus was discovered; the uterine treatment was successful."[b]

Dark vomit—or, as it usually is called, coffee-ground—in midwifery practice is considered indicative of serious derangement, or

[a] Sir H. Marsh's Transmitted Affections of the Stomach.
[b] Observations on Transmitted Disease.

of grave injury to some internal organ. In rupture of the uterus, for instance, it forms a prominent symptom. It was present in Case II. When the patient mentioned having vomited blood before her admission, it was supposed this was owing to the violence with which the contents of the stomach were ejected. However, she made the same remark when in hospital. I never, on any former occasion, saw so large a quantity of this peculiar fluid expelled. In four hours two very large chamber utensils were filled with it. That the danger it indicated was extreme there could have been no doubt, judging alone from the distress, prostration, and agony of the patient. The effect of such a secretion, drawn from the system so largely, was in itself alarming, when, according to the analysis of Dr. Moore, in his examination of coffee-ground vomit, we find it to consist *chiefly of blood*, mixed with epithelium.

In deciding upon the necessity of inducing premature labour, the urgency of the symptoms must be fully considered, so as not to fall into the error of either unnecessary haste or of culpable delay.

A glance at the foregoing case (No. 2) is sufficient to show that in every particular the symptoms were extremely severe. They were numerous, and may be said to fully come up to the signs which, according to M. Dubois, indicate the proper period for resorting to the induction of premature labour.

He states those signs as:—1. Almost incessant vomiting, by which all alimentary substances, and sometimes the smallest drop of water, are rejected. 2. Wasting and debility, which condemns the patient to absolute rest. 3. Syncope, brought on by the least movement, or mental emotion. 4. A marked change in the features. 5. Severe and continuous febrile action. 6. An excessive and penetrating acidity of the breath. 7. The failure of all other means.[a]

Dr. Murphy,[b] in his observations on this subject, well remarks:—"In four cases of excessive vomiting, followed by exhaustion, *three* were lost through hesitation and delay, and *one* was saved by promptitude in inducing miscarriage."

Dr. Churchill[c] mentions a case where, after giving several doses of ergot, and passing a bougie into the uterus, it was four days before the ovum was expelled, and the patient was reduced to a most alarming state of exhaustion.

[a] Gazette Medicale, No. 23.
[b] Murphy's Midwifery. Second Edition, p. 94.
[c] Churchill on Diseases of Women. Fifth Edition, p. 630.

In Case I. the spontaneous coming on of labour was most fortunate, as the patient's state was so alarming that I believe the induction of labour would have been considered imperatively necessary to save her life.

The maniacal attack in this case, where the strength was so reduced by suffering, was a cause of some anxiety; but, coming on at so late a period as the tenth day, and the pulse being so little disturbed, hopes were entertained that it would subside, as it did after refreshing sleep, induced by opium

It has been remarked that extensive tubercular disease of the lungs has sometimes prevented sea sickness in persons who when in health had suffered severely from it. In pregnancy one of the worst cases of sickness I have seen was in a consumptive patient. She, like the one of Case I., was nearly the only member of her family who had not been carried off by it. It was her first pregnancy. She suffered so very much from sickness and vomiting that the induction of labour was in contemplation, but her spontaneous delivery removed the necessity for resorting to this course of treatment. Some months after she died of rapid phthisis.

In the treatment of vomiting, whether of pregnancy or arising from other circumstances, the attitude of the patient is a matter of much importance. M. Beau considers the horizontal position as one of the causes of bilious vomiting, and in his treatment of such cases makes his patients sit upright, with the head and shoulders as much elevated as possible.[a] The horizontal is that preferred in the vomiting of pregnancy, and it is also found of very great benefit, when judiciously adopted, in preventing sea sickness. With the latter object in view this position should be taken before the sailing of the vessel.

Of external remedies ice is one very grateful to patients. It may be enclosed in a bladder, or a piece of it placed in the cup of a sponge, may be as frequently applied over the epigastrium as the disposition to sickness arises. In a case, lately under my care, of violent pain in the stomach and vomiting (not arising from pregnancy), ice used in this way, and given with infusion of mint internally, quieted the attack after the failing of other remedies. It is very pleasant and beneficial when given in coffee.

Creosote and morphia, in combination, internally, are frequently serviceable. They answered remarkably well in Case II., when all

[a] Gaz. des Hôp.—Med. Press, April 27, 1864.

other means had proved unavailing in affording relief from the incessant vomiting, violent straining, and intolerable sensation of burning complained of in the epigastrium; also in procuring several hours' rest in sleep.

Dr. Simpson found the inhalation of laudanum to quiet the severe sickness and vomiting of a pregnant lady, and to procure sleep, after that ice, prussic acid, and other remedies had failed. He recommends oxalate of cerium in various forms of obstinate vomiting. I have frequently given it in such cases with decided advantage, alone or combined with sedatives, such as prussic acid, &c.

Where the strength has been so greatly reduced by sickness and vomiting, extreme caution should be observed as regards diet and the use of aperients after delivery, until the constitutional vigour has become somewhat restored, and the stomach be capable of receiving and digesting suitable food. This precaution was particularly necessary in the foregoing cases.

In Case II. the patient eagerly desired chicken, which she took in preference to any other kind of food. It was given in very small quantity. Broth she disliked. Wine was given, diluted, and little at each time. Aperients were withheld, fearing their effect upon her when so weak, fortunately, as the attack she had of diarrhea exhausted her strength very considerably.

Before delivery had come on in Case I., for some days, when nutriment could not be retained by the stomach, it was thrown into the rectum in very large quantities, and with very decided advantage. This was more easily accomplished after morphia or opium, in any form, had been given, either to procure rest in sleep or to restrain diarrhea.

After delivery, wine was nearly the only thing the stomach could receive for the first two or three days. Champagne and Sparkling Moselle, iced, were particularly grateful.

To enumerate the many and various remedies, both in diets and medicines—together with the numerous external applications which have been resorted to in the sickness and vomiting of pregnancy—would, in itself, testify how very intractable many of those cases are, whilst it would the more enhance the value of a method which, when *timely* and *judiciously* resorted to, has the so very immediate effect of not only rescuing the patient from pain, wretchedness, and, it may be, even death itself, but of restoring a constitution, otherwise healthy, to the proper performance of its wonted functions.

Art. III.—*Brief Considerations respecting the Weights and Measures and the Nomenclature of the Pharmacopœia.*—By T. W. Belcher, M.A., M.D., T.C.D., Fellow of the King and Queen's College of Physicians in Ireland.

As the *British Pharmacopœia* is now an accomplished fact, and as its enactments, whether liked or not, are binding on the profession, I propose to make a few observations on the use of it by the prescriber, having reference to its technical nomenclature, but chiefly adverting to its weights and measures. This I shall endeavour to do in the spirit of a lover of law and order.

The measures remain as heretofore, and therefore the prescriber in this respect acts as he did since 1851.

To the Irish prescriber the weights are, for all practical purposes, as before. In the last *Dublin Pharmacopœia* the following scale is used, the grain being the avoirdupois grain:—

℔. One pound	= 16 ounces	=	7·000 grains.
℥ i One ounce	= 8 drachms	=	437·5 grains.
ʒ i One drachm	= 3 scruples	=	54·68 grains.
℈ i One scruple	=		18·22 grains.

From the ounce downwards this differed from the commercial avoirdupois weight, as seen by the following comparison:—

Dublin Pharmacopœia.	Commercial Avoirdupois.
℥i=8 Drachms=437·5 grains.	1 oz.=16 Drachms= 437·5 grains.
ʒi=3 Scruples=54·68 grains.	1 dr.= 27·3125 grains.

From this it is clear that those who were in the habit of prescribing the drachms and scruples of the last *Dublin Pharmacopœia* can continue to do so if they will; for beside the fact that such weights are in actual use, there is also the other fact that the use of them in no wise alters anything *enacted* in the *Pharmacopœia.* In the preface, the prescriber and dispenser are "recommended" to discontinue the use of them; just as apothecaries are "warned" to "duly alter or destroy *all* pharmaceutical preparations made according to previous and now altered formulæ." Except so far as convenience or discretion may urge persons to adopt this recommendation, or to take heed to the warning, I imagine few will concede to them

(what, indeed, is not claimed) the legal force of the *Pharmacopœia* itself, any more than its compilers would comply with the recommendation or warning of some of their professional brethren to share with them the funds absorbed in its manufacture. It must be allowed that the reason given in the preface for recommending the discontinuance of the scruple and drachm weights is not a fair one:—"Since they can no longer exist as both simple multiples of the latter and integral parts of the former." For the very same reason should the use of the ounce be discontinued; as it is plain 437·5 grains cannot be divided into integral parts, any more than 54·68 or 18·22 grains. For example, the grain is now 2-857ths of an ounce; 10 grains is 1-43¾th of an ounce; 20 grains = 1-21⅞th; and 60 grains = 1-7$\frac{7}{24}$th of an ounce. Others may be thus set down:—

$$\left.\begin{array}{lll} \frac{1}{2} \text{ ounce} & = & 218\frac{3}{4} \\ \frac{1}{4} \quad ,, & = & 109\frac{3}{8} \\ \frac{1}{8} \quad ,, & = & 54\frac{68}{100} \end{array}\right\} \text{Grains.}^{a}$$

In the second part of the *Pharmacopœia* the ½ ounce is frequently used, and how the 218¾ grains can be defended as forming a separate weight, while 54·68 and 18·22 are excluded, is not easy to see. The use of large numbers in grains is also likely to cause no small confusion both in idea and in calculation; take the two following, opened on at random. In the preparation of the green iodide of mercury, the chemist is directed to take of "iodine two hundred and seventy-eight grains." Now, unless he has a bag full of grain weights at hand, and has plenty of time to tell them out one by one, he must resort to a calculation to find what part of an ounce 278 grains is. The *Pharmacopœia* presumes the existence of a half-ounce weight equal to so many grains *and* so many decimal parts of a grain. Let him use this as part of the 278 grains; what, then, is he to do with the decimals when he comes to complete the required number? Also, in the preparation of bismuth lozenges the direction is, "Take of white bismuth 1,440 grains." How is this to be readily got at with only three numbers, 1, 437·5, and 7,000 grains? Much, though not all, of this difficulty will doubtless be got rid of by using weights of multiples of ten.

The confusion set before a student under examination as to a

[a] These numbers are not imaginary; with some others in this paper, they are taken from the most able and friendly review of the Pharmacopœia which has yet appeared—that in the Edinburgh Medical and Surgical Journal for Feb. 1864.

comparison between the weights and measures, may be instanced in the following questions and answers:—

Materia Medica Examiner.—How many grains of opium are in a fluid ounce of the tincture? *Student.*—32·8125 grains.

Question.—How many in a fluid drachm? *Answ.*—4·10156.

It is plain that every practitioner should know the strength of tincture of opium, so that this is not a theoretical, but a practical case. For myself, I use the large numbers of grains as recommended in the Preface, because we all, heretofore, calculated doses in grains, and then rendered them into the condemned weights by a process of mental arithmetic. This has the disadvantages already referred to, but it has the advantages of promoting order and uniformity, and facilitating our use of the grain system in other branches of science. Obviously, the prescriber need not trouble himself about a decimal part of a grain, when the fraction is very small, *he* may ignore it in practice, but the student, or pharmaceutist, should be quite *au fait* in the chapter on decimals in Galbraith and Haughton's Arithmetic.

A few remarks may be made on nomenclature and on the symbols of weights and measures.

It is perfectly open to any prescriber to write prescriptions in the vernacular. The English name of a drug is *now*, for the first time, throughout the kingdom of equal authority with the Latin;[a] and, where any error is likely to arise from using Latin, plain English is undoubtedly the better of the two. When we recollect that any ignorant person may, and many actually do, compound prescriptions in Great Britain, it is plain that the probability of error is not at all inconsiderable; indeed *security* can only arise from the prescriber's personal knowledge of the ability of the compounder. In our own country, however, as we have the apothecary possessing a monopoly of compounding, and as he is educated and legally qualified for the purpose, the danger of error is not by any means so great as among our friends across the water.

A good many among us adopt the *via media*, and write the prescriptions in Latin, while they give the directions for use in English. I think this medical hotch-potch, as a general practice, is unworthy of our profession With many of undoubted ability and

[a] It is true that in the Preface we are told, with reference to each drug, of "a Latin pharmaceutic name by which it may be prescribed." Yet this part of the book is not contained in the Pharmacopœia, which alone is legally binding; and in the latter, as above observed, the English and Latin terms are of equal authority.

education it is pursued deliberately to avoid error; and, in one case of a very able medical friend, to impress the directions on the friends of the patient with whom the prescription is left. In many others it is the result of ignorance; a large number could not, if they would, express their directions in Latin, while a still larger number conceal the weakness of their Latinity under the abbreviations in common use. They would find it an impossible task to write a prescription in full; and the best attempt they could make at a Latin direction would be something like the "*faciebo squarum*" of the fellowship candidate under mathematical examination at T.C.D.

Respecting the signatures to prescriptions, it is not generally known that the following piece of legislation is still in force in Ireland. By the Irish Act of the 1st year of George III, cap. xiv—commonly called Lucas' Act, and made perpetual by 30 George III., c. xlv, sec. 11—it is provided in sec. 20:—"And be it further enacted, by the authority aforesaid, That all and every physician, chirurgeon, or other person or persons, taking upon him or them to prescribe medicines, whether for internal or external use or application for the health of man's body, shall subscribe every such prescription with his name or surname, or the initial letters of his name and surname, and with those also of his profession, whether physician or chirurgeon, and, if a physician, with the name or the initial letter of the name of the university or universities of which he is a doctor or other graduate in physick, under the penalty of forty shillings for every omission, *unless he be a member of the Royal College of Physicians*,[a] in which case the initial letters of his name alone may be sufficient."

By sec. 23, "all the penalties inflicted by this act may be sued for and recovered, in a summary way, before the Lord Mayor and Recorder of the city of Dublin, or the Mayor, or other chief magistrate, and Recorder of any other city or town corporate, where such offences are or shall be committed." One moiety to be applied "for the purchasing medicines for the use of the poor, and the other moiety thereof for the use of the informer."

By a clause in the last Charter of the College of Physicians it is provided that no apothecary, in Dublin or its neighbourhood, shall "take any Apprentice or Apprentices but such as shall be so well skilled in the Latin Tongue as to be able to understand

[a] *Member* in this clause means *Fellow*.

such Prescriptions as by any such Physicians shall be sent unto them in Latin." The candidates for apprenticeship were to be examined by the President and Censors of the College as to their Latinity, but this practice is now exercised by the Governors of the Apothecaries Hall. It is obvious that if the spirit of the preceding rule were carried out there would be such correct pharmaceutical Latinity as to leave no pretext for a practitioner prescribing, either wholly or partly, in English.

If we are to continue the use of Latin prescriptions I think a prominent place in all final medical examinations should be given, not only to pharmaceutical Latin, but also to the art of writing the same with a trifling measure of legibility.[a]

Before saying more on this subject it may be well to notice the symbols inserted in the Preface to the *Pharmacopœia*, or, rather, the abolition of those previously in use.

If a prescription be written in English the present system is generally consistent with itself; but if in Latin I have no hesitation in calling it clumsy and ungrammatical in the extreme. Take the following example, in which the weights are used:—

℞—*Unguenti simplicis*, ℔ i (*libram unam*).
Gallæ pulveris, oz. i (*onze unam*).
Opii pulveris, gr. 10 (*grana decem*).

Here ℔., being itself a symbol of the pound avoirdupois, and also an abbreviation of the Latin *libra*, a pound, its use, whether with Roman or Arabic numerals, is quite correct; but in what light is oz. to be considered? If as a symbol, it does not answer the purpose, because though it has a verbal meaning it is not a Latin one. If, on the other hand, it be considered an abbreviation, we have the French "once," in its old spelling, coupled with the accusative case of a Latin pronoun; while in the third instance ("gr. 10"), if it mean *grana decem* it is good Latin; but, if it be English, "grain ten" is bad grammar. Should the English be corrected, and the new reading be grs. 10, you have the unclassical mixture of English and Latin.

[a] The recent decision of the *intelligent* (?) majority of the Medical Council to the contrary notwithstanding, it is most desirable that every student should know Greek, and medical Greek, too. In no Irish preparatory school of any pretensions is a youth taught Latin without being taught Greek also. However, it seems that the educational standard of our profession must be lowered to suit the Grecian famine among our friends across the water.

Again, with regard to the measures, there is the same objection. A gallon has a symbol, C, which in a Latin prescription is *congius*, and a pint is O, in Latin *octarius*. So far the use of these in Latin prescriptions is correct; but we next come to *fl. oz.* and *fl. drm.*, evidently intended as English symbols or abbreviations, or both. For the reasons already stated these are manifestly incorrect in a Latin prescription while the *minim* retains its former position.

It will be observed that the Council retain the Roman numerals for fluids, while the Arabic are used for solids. This is a very useful distinction; but, then, in a Latin prescription you have anything but a pleasing mixture of English, old French, and Latin, *e.g.*, fl. oz. xx., "fluid *onze viginti*." The fl. is manifestly superfluous where the very character of the numerals is sufficient distinction.

In the preface of a very useful little book, *The Prescriber's Companion*, by Dr. Meadows, without which, or some other like work, the *Pharmacopœia* would be a sealed book to the practitioner, it is suggested to retain the abolished symbols ʒi and ℥i for fluids, instead of fl. oz. and fl. dr., prefixing f to each, for distinction's sake. The following specimen prescription is given in illustration of the system there proposed:—

℞—*Ammoniæ carbonas*, grs. 30.
Spiritus chloroformi, f. ʒi.
Aqua camphoræ, f. ℥viii.—M. *ft. mist.*

Now, over and above the fact, which every schoolboy can discover, that each line of this prescription is wholly defiant of the genitive case, into which *carbonas*, *spiritus*, and *aqua* should have been declined, we object to grs. 30, that, whether it be understood to represent "grains thirty" or "grains *triginta*," it is equally bad in a Latin prescription; while to f. ʒi and f. ℥i it is objected that, whether rightly or not, these symbols no longer exist, having been abolished by law.

In what way, then, can we continue correct Latinity and yet write a prescription in accordance with the symbolism of the *Pharmacopœia?* Only by verbal description of weights and measures, where the symbolic plan would prove clumsy and incorrect. Thus C, O, and ℔. are themselves symbols of Latin terms, and may be correctly used; so may gr. and min., which may be singular or plural and yet good Latin; while the addition of s to either Anglicises it. So drm. may stand for *drachmam*, and drs. for

drachmas, and be so written. The ounce is the weak point; and to illustrate the plan just alluded to I shall apply it to prescription No. 302 in the fifth edition of Hooper's *Physician's Vade Mecum* :—

℞—*Potassæ tartratis, unciam.*
Mannæ optimæ, unciam.
Aquæ destillatæ, fl. uncias vi.—M.

This is in perfect accordance with the symbolic system of the *Pharmacopœia*, and it is manifestly clear enough. Now, I shall put the question—" Apply this principle to the prescription quoted from Dr. Meadows, having special reference to its Latinity?" The answer may be best given by marking the quoted prescription A, the altered one, B, and then placing them in parallel columns:—

A	B
℞—*Ammoniæ carbonas*, grs. 30.	℞—*Ammoniæ carbonatis*, gr. 30.
Spiritus chloroformi, f. ʒi.	*Spiritûs chloroformi, fl. drm.*
Aqua camphoræ, f. ʒviii.—M.	*Aquæ camphoræ, fl. uncias* viii.—M.

After all, the prescription B is not written with symbols, but with abbreviated words; and there is no enactment in the *Pharmacopœia* for the use of symbols of any kind in prescriptions. Those laid down in the preface may be considered as useful to the chemist and pharmaceutist; while it is to be observed that in the work itself the quantities and numbers are set down in words at full length. Thus we may prescribe, as above laid down, in perfect consistency with the symbolism of the *Pharmacopœia*, by using abbreviated *words*. This system will answer well for printed formulæ in medical books and for private case-books; but for the Irish practitioner there is one course open in writing prescriptions for apothecaries. It combines clearness, correct Latinity, obedience to the provisions of the *Pharmacopœia*, and to the existing law of the land; for section 19 of " Lucas' Act," already quoted, provides:—" And, in order to prevent the uncertainties and dangers which may attend the setting down the quantities of medicines in chemical and numeral characters, in prescriptions, be it enacted, by the authority aforesaid, That every physician, chirurgeon, or other person or persons, who now do, or hereafter shall, take upon him or them to prescribe internal or external remedies for the health of man's body in this kingdom,

shall hereafter write or set down the quantity or quantities of all and every medicine or ingredient, whether simple or compound, which he or they shall prescribe in any *recipe*, *formula*, or prescription, in words at length, and not in chemical or numeral characters, under the penalty of forty shillings for every such omission."[a]

The quotation of the carbonate of ammonia above is an instance of one of the grammatical changes wrought in the *Pharmacopœial* nomenclature. I refer to the fact that the specific names of salts, which were formerly *neuter* in the Dublin and Edinburgh *Pharmacopœias*, with a short penult in their inflections, are now feminine, with inflected long penult. Thus carbonas, which in Dublin and Edinburgh became carbonătis in the genitive case, is now declined carbonātis, as formerly in London.

After reading many criticisms on the *Pharmacopœia*, and many comparisons of it with past editions, I believe the only way in which the practitioner can safely act is to wash the tablets of his memory as clean as Locke's celebrated sheet of white paper, so far as all past *Pharmacopœias* are concerned, and to "make up," *modo scholarium*, the new one, as a previously unexplored field, a *terra incognita* in *Materia Medica*. Until this is done, the best plan is to prescribe *Secundum Pharmacop. Dub.*, and head the prescriptions accordingly. Dr. Watson has been strongly censured for recommending his brethren to adhere to the old until they learned they new;[b] but what else can be done by busy men, who are expected to act forthwith according to the directions of a book, which, after long deferred years of preparation, is suddenly thrust forth from the press without—what certainly should have been given—six or twelve months' notice of its edicts coming into operation.

This is the only part of Dr. Watson's now celebrated speech which can commend itself to loyal members of the profession. I refer, of course, to a late meeting of the College of Physicians of London, where its eminent President, in effect, advised the Fellows to ignore the existence of the *British Pharmacopœia*, and to prescribe as heretofore. I wonder what we should all say to the Provost of Trinity College, if he should address the Fellows of that body, and recommend them to ignore the existence of an Act of Parliament concerning education, founded on the report of a commission of

[a] I am indebted to the kindness of Professor Aquilla Smith for copies of the legal documents noticed or quoted in this paper.

[b] The newspaper reports of Dr. Watson's recommendation describe him as censuring the Pharmacopœia, while admitting that he had not read it!!!

educated men? Yet such an example as this has been shown on a grave matter concerning the public health, by the head of what was always considered the first medical corporation in law-and-order-loving England. Doubtless it is not pleasing to the strong prejudices of medical men who have thought and written on every conceivable professional question, as if London were the kingdom, and no facts or knowledge outside it were worthy of notice, to be called on to use the large part of the last *Dublin Pharmacopœia*, which has been virtually incorporated with the new code. A local prejudice on the part of the profession, and a trade jealousy in another direction, will go far to explain the *hinc illæ lachrymæ* touching the *Pharmacopœia*, which even with a camel's load of errors, as some have it, is a great step in the right direction.

That apothecaries and chemists will destroy *all* old preparations is not to be expected or *wished*. What would become of family prescriptions of men deceased—say of Graves or Marsh—in this city? Any man reading a good book, published before this *Pharmacopœia*, will have to go through the labour of translating the prescriptions in it into the language of novel pharmacy. In the late French edition of Graves' *Clinical Medicine*, the prescriptions are Gallicised in the foot-notes; the same thing must be done with the last English edition. If those who write for students or junior practitioners, do not adopt the *Pharmacopœial* language and symbols they will only confuse the very class they design to teach.[a]

Art. IV.—*On the Therapeutic Value of the Alkaline and Earthy Sulphites in the Treatment of Catalytic Diseases.* By H. R. de Ricci.

It is now about three years since my distinguished friend, Professor Polli, of Milan, communicated to the world his important discovery of the action of the sulphites in certain forms of disease. Being fully impressed with the value of his discoveries, I have ever since devoted much of my time and attention to the careful investigation of the therapeutic action of these salts, not only by trying experiments upon the lower animals, but also by largely administering

[a] This is not theoretical. Sometime since the appearance of the Pharmacopœia, Dr. Frazer, himself a well-known writer on, and teacher of, Materia Medica, published a useful book on the treatment of skin diseases, in which the abolished symbols are used. In the preface this book is recommended to students and junior practitioners.

them in my practice in every case that seemed likely to be benefited by them. The results have been most encouraging; and I now submit them to my professional brethren, by whom I trust that the experiments will be repeated, when I feel certain that they will obtain equal, if not greater, results.

For the benefit of those who may not have read my former publications on the action of the alkaline and earthy sulphites, nor the original writings of Professor Polli, I shall now, as briefly as possible, recount the origin and result of his discoveries.

For several years past a theory has been almost universally gaining ground that a great number of diseases depend essentially on the presence of an organic poison circulating in the system, where, acting as a ferment in the blood, it multiplies itself, vitiating the animal fluids and giving rise to divers diseases, according to the special poison in circulation; thus in one case producing small-pox, in another scarlatina, in another puerperal fever, &c. These diseases have been termed *zymotic*, on the supposition that the circulating poison acted as a *ferment* in the blood; but, as the precise mode of action of these animal poisons is not as yet well understood (though it is no doubt a chemico-vital action), I shall throughout this paper employ the term *catalytic* instead of *zymotic*, as perhaps better calculated to convey the idea of the function of these poisons, without absolutely involving a principle, which the term zymotic might do.

Several years ago Professor Polli, being fully convinced of the truth of the catalytic theory of many diseases, devoted his time and talents to search out some means by which these poisons, even after they were absorbed or developed in the animal economy, might be neutralized, and eventually eliminated, without, at the same time, imperilling the integrity of the blood. The task was an arduous one, for the great physiologist, C. Bernard, had dogmatically asserted that any substance capable of destroying a catalytic poison in the blood would, at the same time, so alter that fluid itself that it would be rendered incapable of performing its vital functions. Professor Polli, however, had long observed the action of sulphurous acid in arresting the process of fermentation. He was convinced in his own mind that in it would be found the anti-catalytic substance he was seeking for. He persisted in his researches, and eventually succeeded in establishing that not only sulphurous acid possesses the property of arresting fermentation and neutralizing catalytic action, but that also its alkaline and earthy compounds have the

same power, with this difference, that while sulphurous acid can not in any way be introduced into the circulation with safety, the alkaline and earthy sulphites, hyposulphites, and bisulphites can be administered with the greatest impunity, even in large doses; and their presence can always be detected when administered, not only in the blood but in the tissues and evacuations. Having satisfactorily established this important fact, he began a series of experiments on animals, which proved, most successfully, the correctness of his theory and the accuracy of his deductions. Nothing could give me greater pleasure than to recount the several interesting experiments which he performed, but it would lengthen this paper too much, and otherwise occupy too much of the space of the Journal; and, for those who are interested in this matter, I beg to refer them to a communication read by me, about a year ago, at the Obstetrical Society in this city, in which will be found a short epitome of Professor Polli's experiments.

From the moment I first read the account of Professor Polli's researches I came to the conclusion that his discovery was either valueless or of immense value. In such a case there could be no medium; it was either utterly worthless, or it was a discovery second to none—a discovery even greater than that of vaccination, and one which would constitute one of the greatest epochs in the annals of medicine; for vaccine could, at most, only ensure the vaccinated against one single disease, while the sulphites—if they truly could neutralize the action of catalytic poisons—of such poisons as *glanders* (see Polli's experiments)—would be able to arrest the course of small-pox, hydrophobia, syphilis, infection from dissecting wounds, erysipelas, puerperal fever, measles, scarlatina, whooping cough, dysentery, diarrhea, cholera, influenza, typhus, dothinenteria, plague, diphtheria &c., &c., all which are classed by Dr. Farr as zymotic diseases. The question seemed to me of such vast magnitude and importance that I solicited in every direction for co-operation in testing the real value of these sulphites. I first tried them on myself, and finding them decidedly harmless, even in doses of one scruple, five or six times a-day, I began to administer them in every case where I thought that a catalytic poison might be the cause of disease. In no case did I meet with any mishap, and in most cases I think I can fairly say I was successful. In no case did the exhibition of the sulphites produce sickness of stomach, diarrhea, or any other inconvenience; and in every case their administration seemed to be of decided advantage.

The first case I shall describe is one of great interest and value in a clinical point of view; it was undoubtedly a case of infection from an animal poison. The patient was almost given over as lost, so severe were the symptoms of the disorder; yet the patient recovered, and the treatment consisted solely in the administration of the bisulphite of soda, in full, repeated, and continued doses.

A lady of about forty-five years of age, of sound constitution, and in the enjoyment of excellent health, was suddenly called, about a year ago, to the death-bed of one who was very dear to her. That death-bed was fearfully sudden and unexpected, and that poor lady could not be persuaded, long after death had indubitably taken place, that the spirit of the beloved one had really fled. She would not leave the corpse; she threw herself on it, and kissed it over and over again, and could not be induced to leave it, even when the discolouration of the skin and the offensive smell of rapidly-advancing decomposition gave ample testimony of the reality of death. The burial was performed two days after death, owing to the rapid decomposition of the body; and, soon after the funeral, I was hastily summoned to the bedside of this lady, whom I found in the following condition. It was about five in the morning when I entered the bedroom. She had gone to bed the night before quite calm and resigned, and on the previous day she had partaken fairly of food, but had not eaten anything which could in any way account for the state in which I found her, of which the following is a fair sketch:—The windows of the bedroom being open, the morning light streamed freely into the room, and as I approached the foot of the bed I had a full view of the patient's face. To those of my readers who, like myself, have been familiar with the victims of cholera I shall simply say that the patient looked like one in cholera, in the stage of collapse. To those who have not yet had the melancholy opportunity of witnessing a case of that terrible disease I shall say that I hardly recognized my friend, so altered, and pinched, and ghastly were her features. Her eyes were sunk, and surrounded by a lead-coloured zone; her cheeks, which, but a day before, were plump and ruddy, were now hollow and sunk; her eyes were glassy; her pulse scarcely to be felt; the surface of the body cold; her breath cold; her tongue cold; her voice low and husky; her faculties perfectly clear. She now lay quite prostrate on her back; but for some hours previously had suffered greatly from vomiting, cramps, and diarrhea, which was of the characteristic rice-water appearance always observed in cholera. I at once

administered a full supply of hot brandy and water, ordered turpentine stupes to the abdomen and limbs, treating the case exactly as one of Asiatic cholera; and, being fully alive to the grave nature of the attack, requested for further advice, and at once sent off for my valued friend, the late Dr. Mayne. At first I felt singularly at a loss how to account for so sudden and so severe an attack of what seemed to be a genuine case of Asiatic cholera. So true was it to symptoms that, when Dr. Mayne looked at the patient, he whispered to me:—"If cholera were in the country one would not hesitate to give this case a name." The clue to the disease was, however, given to me, in a few words, by the patient herself. As soon as she had taken the brandy she said to me:—"The smell of the body was dreadful; I cannot get rid of it in any way;" and immediately she began to retch. She had given me the key, and the mystery of her case was solved. It was no doubt a case of putrid infection—of septicemia; and if there was any truth in the "sulphite" theory it should prove of value in such a case as this. I at once ordered a strong solution of bisulphite of soda in infusion of quassia, with tincture of bitter orange peel and Battley's sedative—two drachms of the bisulphite to the ounce—and gave it in large teaspoonful doses, every half-hour at first, and then every hour, each dose containing nearly twenty grains of the bisulphite. I watched this case incessantly myself, day and night, and the result was most satisfactory. All the symptoms by degrees abated, and in a very few days the patient was fully convalescent. Dr. Mayne, who had anxiously watched this case with myself, was so impressed with the results obtained that he told me he would give the remedy a full trial in every case of scarlatina that should come under his care; and, in addition, promised (at my suggestion) that he would prescribe a dose of the sulphites daily to every healthy member of the family in which there was a case of scarlatina, to test its value as a prophylactic; for if a sulphite could destroy a catalytic principle, even when developed in the system, it should also have the power of preventing the development of that principle from the commencement.

As I stated above, the lady recovered, and, apparently, was restored to absolute health; she, however, complained to me occasionally of wandering pain, and general *mal-aise* at times, which she could not account for About five months after her recovery she hurt her leg; it was a very trifling abrasion, but it assumed an angry look, and seemed determined not to heal, when, at the end

of two months, she broke out all over with an extraordinary eruption, more like erythema-nodosum than anything else, when I at once placed her again on bisulphite of soda, in the supposition that some of the poison was still lurking in her blood, when the sore in the leg rapidly healed and the eruption disappeared, leaving the patient perfectly well.

The next two cases in my list were well-marked types of measles. I shall not take up the time of my readers by giving the details of them. I shall merely state that they were severe cases; that they were treated solely with bisulphite of soda, in scruple doses, every second or third hour, and that both cases grew rapidly well.

The fourth case is one of poisoned wound. A. B., aged about thirty-five, a gardener, was grafting a cactus, and in carelessly handling such a thorny plant, got the back of his left hand severely stung in different places. He plucked out as many of the spines as he could discover, and thought no more about the matter; but in the course of twelve hours or so the hand began to swell and be painful. He at once wrapped it up in a poultice; but the pain and swelling not abating, his employer sent him to me, three days after he had been stung. I found his hand enormously swelled, of a dusky purple colour, with large bullæ over the dorsum; the fore-arm also swelled, though not discoloured, but presenting several longitudinal red lines running up to the elbow. The man complained of great pain locally, of intense thirst, headache, loss of appetite, shivering, and general feeling of sickness. His tongue was furred and brown. He had, of his own accord, taken a dose of senna and salts that morning. I at once made an incision in the dorsum of the hand, and some pus came out. It was not, however, like cutting into an abscess—no gushing of matter took place, only an oozing, like as if I had cut into a sponge saturated with pus. I therefore made a second incision, parallel to the first, from which some more pus came out; ordered him to wrap up the hand in a large linseed-meal poultice, and prescribed the bisulphite of soda, in scruple doses, every second hour. I desired the man to keep quiet at home—without, however, ordering him to keep his bed; directed him to take light nutritious food, and to drink two pints of XX porter in the day. The following day the hand looked better; it was less swollen and purple, and the pus was in larger quantity and better looking. The man felt also better; he had less thirst, and had only had two slight shiverings. The bowels not having been freed, I ordered him some sulphur, with magnesia and

scammony, as an aperient, and to persist with the bisulphite, taking it now every third hour. The following day the man was so much improved that he called on me. The hand looked still better; the incisions which I had made had ulcerated somewhat round their edges, but the suppuration was free, and the pus seemed quite healthy; there was no pain in the arm, and the red lines were very much paler. Matters looking so very much better, I desired the man to take the sulphite only three times in the twenty-four hours; to take a good allowance of food; to stay much in the open air; but to still keep the arm in a sling, and the hand wrapped in a poultice. Two days later he again came; when, finding that all swelling had subsided, all pain gone, all red lines disappeared, the sore inclined to granulate, I stopped the poultice, and desired him to dress the hand with some warm dressing, containing a little balsam of Peru, and to take one scruple of the bisulphite twice a day. Two days after the man returned to me, not looking as well as at the previous visit. He had a yellowish tinge in his skin; his tongue was foul; he complained of chilliness, almost amounting to shivering, and the sore at the back of the hand looked unhealthy and angry. On stripping the arm, the red lines were again visible, but of a very pale red; while above the elbow, from its bend to the axilla, a hard, knotty, and extremely painful cord could be felt and seen running up parallel to the brachial artery. I at once returned to the primary doses of bisulphite, wrapped up the hand in a poultice, and watched to see what the result would be. The man immediately began to mend, and in four days not a trace of hardness was to be felt. Fearing, however, a relapse, I continued administering four scruples of bisulphite daily for ten days longer, by which time the hand was perfectly healed, and the man returned to his work.

Two better cases for testing the effects of sulphites could not possibly have been selected; in both cases the disease clearly resulted from the working of a poisonous element in the blood, evidently introduced from without in the first case, whilst it may in the second case have originated within the system subsequent to the stinging with the thorns, if one does not feel justified in considering cactus thorns as poisonous of themselves; be this, however, as it may, the second case was as clearly one of purulent absorption as the first was of putrid infection—the red lines up the arm marking the course of the inflamed lymphatics as the hard knotty condition of the veins denoting the phlebitic inflammation. Both were treated solely with sulphites, and both completely recovered. It would be

great presumption, no doubt, to say that in both cases the patients would have lost their lives except for the saving properties of the bisulphite of soda administered; but I still cannot avoid believing that the sulphurous acid did prevent the spreading of the catalytic principle by rendering it incapable of re-producing itself;[a] and, while keeping it in abeyance, allowed time for its elimination by the ordinary powers of nature; and I think we have a confirmation of this view in the occurrence of a relapse in both cases, where it would appear as if, when the bisulphite was stopped, all the poison had not yet been eliminated, the blood disease breaking out again the moment that the poison was freed of its antagonist; and a cure being effected by administering more of the anti-catalytic remedy until every trace of the animal poison was eliminated.

(To be continued.)

Art. V.—*On English and Continental Spas, their Physiological Effects, and their Therapeutical Use.* By Julius Althaus, M.D., M.R.C.P., Lond., Physician to the Royal Infirmary for Diseases of the Chest.

This paper is intended to be a guide to the practitioner in prescribing those mineral waters of England and the Continent of Europe, which are at present generally used for therapeutical purposes. I shall successively consider the alkaline acidulous springs, the muriated alkaline acidulous waters, the alkaline saline, and the bitter waters; the simple muriated and the muriated lithia waters, the brines, the iodo-bromated and earthy waters, the indifferent thermal springs, the chalybeates, and the sulphurous waters.

1. *Alkaline Acidulous Waters.*—The chief ingredients of these are carbonic acid and bicarbonate of soda, and the most important spas of this class are Vichy, in France, Fachingen and Geilnau, in Nassau, and Bilin, in Bohemia. The quantity of bicarbonate of

[a] The action of the sulphites on zymotic or catalytic principles is very peculiar; it does not seem to kill them outright, but only to neutralize their action for the time being, so that in treating a case depending on a catalytic poison a relapse may be surely expected if the remedy is discontinued too soon, and before the powers of nature have been able to eliminate it completely from the system.

soda contained in the several springs amounts to from 8·14 to 37·50 grains, and the carbonic acid from 10 to 47 cubic inches in the pound of water. Their therapeutical effects are mainly due to the water, its temperature, and the two ingredients just named.

Carbonated water, when drunk, quenches thirst and has a pleasant effect upon the palate. Carbonic acid being the chief spice contained in cold drinks, especially in fermented liquors, water has a very flat taste when devoid of this gas; and even breast milk is rendered more palatable by it to the infant. Carbonated water stimulates the gustatory nerves, increases the flow of saliva, and thereby promotes digestion; at the same time the earthy salines, which are contained in the food, are more easily dissolved. Part of the gas is generally brought up by eructation, especially if the stomach is full and the quantity of water drunk considerable. Another part is absorbed and carried to the portal vein and the liver, and afterwards to the brain and other remote organs When taken in moderate quantities, acidulated waters accelerate the pulse, render the head clear, aud the mind cheerful. Large doses, such as from ten to twenty tumblerfuls, cause sickness, vomiting, and congestion of the brain; and unless the carbonic acid is quickly eliminated, apoplexy may be the result. In such cases the physiological effects of the gas are combined with those of a mechanical distension of the stomach, whereby a pressure upon the heart and the great vessels is produced.

Bicarbonate of soda when internally administered exalts the natural alkalinity of the blood and renders the urine alkaline. It also increases the quantity of excretions, and promotes menstruation.

The taste of the alkaline acidulous waters varies according to the quantity of carbonic acid, and solids contained in them, and is pleasant if the former, and unpleasant if the latter predominate. They increase the appetite, but scarcely ever act as aperients. A diuretic action is the rule; but perspiration is generally not increased. The mucous secretion of the bladder becomes less tenacious. Their action upon the urine differs according to the state of the stomach, and that of the system generally. In persons suffering from excessive acidity in the stomach, the alkali is neutralized in the primæ viæ, and it therefore does not alter the reaction of the urine. But where little acid is present in the stomach, as, for instance, in a healthy person before breakfast, the urine quickly becomes alkaline. Large doses produce irritation and inflammation

of the stomach and the kidneys, whereby the urine is prevented from becoming alkaline. Thus a small quantity of Vichy water may cause the urine to become alkaline; while, if a large amount is taken, it may remain acid. A too much prolonged use of these waters has frequently a noxious effect upon the composition of the liquids and tissues of the body, and may induce general prostration.

Acidulous alkaline springs are useful remedies in those forms of indigestion which arise from deficient innervation, and secretion of the gastric juice, or from chronic inflammation of, and excessive acidity in, the stomach. Carbonic acid improves the innervation and secretion, while, by the bicarbonate of soda the surplus acid is neutralized, and the tenacious mucus, which, in cases of chronic catarrh adheres to the mucous membrane, is fluidified. In icterus arising from catarrh of the hepatic ducts and gall-stones the waters of this class frequently prove curative, especially where there is no disposition to congestion and hemorrhage, nor any great excitability of the nervous system. In gout the waters have not quite answered the expectations that were entertained of them for this affection; but, if the dyspeptic symptoms are very prominent, they deserve a trial. In uric acid diathesis, and in renal calculi and gravel, they are useful, especially in combination with carbonated water baths.

Their reputation as solvents of stone in the bladder is totally unjustified; but in atony of the bladder in old people they may serve as diuretics, and so act as preventives of calculus, as they do not allow an accumulation of mucus in that organ, which might become the nucleus of stone. In chronic bronchitis they are often employed, but are far inferior to the muriated acidulous alkalines; while in portal plethora, on the whole, the alkaline salines are preferable. In diabetes, especially if it is connected with gout and disordered liver, the Vichy waters have frequently a beneficial action; and although it is scarcely to be assumed that they should ever prove curative, yet, by their use, all the symptoms of diabetes have been made to disappear for years.

Baths of Vichy water render the urine neutral or even alkaline; and if they are used for some time the perspiration and saliva also become alkaline. This is most probably due to the local influence of the water upon the skin; the absorption of bicarbonate of soda from the bath being exceedingly doubtful. These baths are useful in most cases in which the water is internally administered.

2. *Muriated Alkaline Acidulous Waters.*—The chief constituents

of these are carbonic acid, bicarbonate of soda, and chloride of sodium; and the most important spas of this class are—Ems, Selters, and Weilbach, in Nassau, Salzbrunn, in Silesia, and Luhatschowitz, in Moravia. The amount of bicarbonate of soda contained in them is from 7·37 (Weilbach) to 44·21 (Luhatschowitz); that of chloride of sodium, from 9·77 (Selters) to 33·47 (Luhatschowitz); that of the carbonic acid, from 6 (Ems) to 50 cubic inches (Luhatschowitz); the temperature is from 45° (Salzbrunn) to 117° 5′ (Ems, Neue Quelle); so that in this as in the former class, there is a great variety of similar remedial agents at our disposal.

Chloride of sodium essentially promotes digestion. It not only causes a more considerable secretion of gastric juice and bile, but it also facilitates the absorption of chyle by the capillary vessels of the intestines; and is effectual in preventing fermentation and putrefaction in the intestinal canal. Moreover, as the phosphate of lime is more easily soluble in a solution of chloride of sodium than in ordinary water the absorption of this important substance is facilitated. The feces are rendered more fluid and are more easily voided, there being a more abundant secretion of the intestinal mucus, and desquamation of epithelial cells. The formation and elimination of urea is augmented, and the retrogressive metamorphosis of matter altogether promoted.

By the internal administration of Ems water the urine is generally rendered alkaline after a certain time. Sometimes its use is followed by considerable diuresis and diaphoresis, while at other times purging is induced. If the weather is damp, diuresis is increased; but in dry weather, and in persons whose skin acts freely, the water has a more diaphoretic effect. Its action depends also in a great measure upon the quantity and temperature of the water taken. It also excites the motion of the ciliated cells and of the spermatozoa; from which it has been concluded that the beneficial effect of Ems in chronic bronchitis may be due to the influence of the water upon the epithelium of the air-passages, and that the effect of the same spa in curing sterility may be ascribed partly to a diminution of the acidity of the vaginal and uterine mucus, which, according to Donné, is very detrimental to the viability of the spermatozoa; and partly to the direct action of the water upon the spermatozoa themselves.

The waters of Ems are the only thermals of this class, and are, on account of their high temperature, useful in many cases where

cold springs of a similar character are not applicable. Bronchitis, if caused by cold, or when it occurs in children after measles, or in gouty persons, where it sometimes alternates with true fits of gout, is frequently cured at Ems; but this spa should be avoided where there is a tendency to hemoptoe and tuberculosis. Obersalzbrunn has a special reputation in cases of bronchitis connected with hemorrhoidal complaints, and Luhatschowitz, where there is a complication with scrofula. The same holds good for laryngitis. In emphysema, by the use of the same spas, dyspnea is relieved, expectoration promoted, and the abdominal circulation improved. In dyspepsia dependent upon disturbances in the portal circulation, in chronic intestinal catarrh, where constipation alternates with diarrhea, in certain forms of jaundice and hemorrhoidal affections, the spas of this class are also often used, although they are in this respect inferior to Carlsbad, Marienbad, and other springs. In chronic inflammation of the bladder Ems is preferable to Vichy, if, the patients are of delicate constitution, and for whom the Vichy waters would be too potent. Gravel and renal calculi are frequently discharged during the use of the Kesselbrunnen, of Ems. In gout these waters may be used, where true fits of gout do not take place, and the patients are weak and irritable. Ems possesses the reputation of being almost a panacea for chronic parenchymatous metritis and allied diseases of the womb; and there is no doubt that in a great number of such cases the internal use of this water and the Bubenquelle, administered in the form of the ascending douche, prove curative after every other method of treatment had failed. The same spa is often useful in hysterical convulsions and paralysis, and in some diseases of the skin, such as eczema, occurring in young women who suffer from difficult menstruation; and in certain cases of prurigo.

3. *Alkaline Saline Springs.*—The chief contents of these springs are bicarbonate and sulphate of soda. The quantity of the former substance contained in Marienbad (Kreuzbrunnen) is 12·39; in Tarasp, 27·22; in Carlsbad (Sprudel), 9·06; of the latter Marienbad contains 36·26; Carlsbad, 19·96; and Tarasp, 16·54 grains saline in the pound. The temperature of the Tarasp springs is 37°; in the several waters of Marienbad it varies from 43° to 53°; and in those of Carlsbad from 117° to 162°.

The taste of Carlsbad water is slightly saline and alkaline, and resembles that of weak chicken broth. If it has been standing for

some time the taste becomes more strongly alkaline, and consequently more unpleasant. If a moderate amount of it is drunk, no striking effects of any kind are produced in healthy persons. The water seldom causes sickness. In former times, when it was customary for patients to drink from three to ten quarts of Sprudel per diem, and even more, and when it was not taken in the immediate neighbourhood of the spring, but carried into the house of the patient, where it was only drunk after having become cold, and lost its carbonic acid, purgative effects were more frequently observed than they are now-a-days; and Carlsbad has for this reason unjustly acquired the reputation of being a strongly aperient water. In many persons no purgative effects whatever are produced by it; and even the addition to it of Carlsbad salt is not always sufficient for relieving the constipation which sometimes follows. In most cases defecation is moderately increased, while in some there is obstinate costiveness, so that it is necessary to clear the bowels by enemata or purgatives. In some persons Carlsbad causes salivation, even when mercury has not been previously given. The quantity of the biliary constituents of the feces is generally augmented. The feces frequently appear black in the second week of the treatment; they afterwards become green, and then brown, and towards the end of the course their colour is again dark green. Sexual desire is sometimes increased; but diuretic effects are scarcely ever observed. The urine is generally alkaline in the morning, a short time after the water has been drunk; but that discharged in the evening and during the night is always acid.

The specific gravity of the urine is diminished in some, while in others it is increased; the elimination of urea and uric acid is considerably diminished; phosphoric and sulphuric acid are generally increased, while chloride of sodium is diminished. The weight of the body is almost always increased.

Patients suffering from disturbance of the abdominal circulation are those most frequently sent to the alkaline saline spas, and are generally improved or cured, if the stagnation of blood is owing to habitual constipation, pressure from accumulated feces, and certain diseases of the liver; especially if brought on by excessive eating, drinking, or smoking, and by sedentary habits or the effects of climate. In cases of abdominal plethora, the action of the skin is generally very sluggish, and baths of mineral water, moor-baths, or moor-cataplasms, are therefore an excellent auxiliary to a cure. In moor-baths the body is able to bear a far higher degree of heat

than in ordinary water or mineral water baths. In obstinate constipation Marienbad is preferable to Carlsbad, as it contains a more considerable amount of sulphate of soda, whereby it is rendered somewhat similar to the bitter waters; from these latter it is, however, distinguished by containing bicarbonate of soda, carbonic acid, and bicarbonate of iron. These ingredients have the effect of accelerating the circulation and increasing the retrogressive metamorphosis of matter, while, at the same time, the system is not so much weakened as is done by the use of the stronger bitter waters.

In hyperemia of the liver arising from abdominal plethora or from ague, the Carlsbad Sprudel is an excellent remedy. The same may be said of fatty degeneration of the liver when not combined with other structural diseases, and granulated liver in its first stage, when this organ has not yet become atrophic. Icterus arising from catarrh of the hepatic ducts, gall-stones, or hyperemia of the liver, are often cured by the same waters; while in cases which are consequent upon granulated liver in the second stage, cancer and other tumours pressing upon the ducts, the use of the waters must be avoided. The discharge of gall-stones during the administration of Carlsbad water is an event of very frequent occurrence. Similar effects are also produced in renal calculi and gravel. In diabetes the same water deserves a fair trial. We know of no radical cure of diabetes by this treatment, but the patients are often temporarily restored to perfect health, especially if this disease is complicated with gout and disordered liver. Great and lasting effects may also be expected from these springs in those forms of gout in which dyspepsia, hyperemia of the liver, and abdominal plethora are prominent symptoms.

The question often arises, whether a patient had better be sent to Vichy, Carlsbad, Marienbad, or Tarasp, as all these springs may promise to be beneficial in certain cases. From Vichy, Carlsbad is distinguished by the absence of any considerable amount of carbonic acid, and by the presence of sulphate of soda. The function of the stomach is therefore more excited by Vichy than by Carlsbad water; and, on the other hand, the latter can be advantageously used in subacute inflammation of the stomach and the intestines, in which the former is likely to do harm. By the sulphate of soda it contains Carlsbad water acts more powerfully upon abdominal circulation than Vichy water. Marienbad is preferable to Carlsbad in cases where an accumulation of fecal matter is the cause of the

disease, and a thorough clearing of the bowels is advisable; and also, for stout persons who suffer from the effects of too generous living. Tarasp is, by its chemical composition and the low temperature of the water, more closely allied to Marienbad than to Carlsbad, and may be employed with advantage in all cases in which Marienbad is suitable.

4. *Bitter Waters.*—The chief contents of bitter waters are the sulphate of soda and magnesia; and the most important springs of this class are those of Püllna, Sedlitz, and Saidschütz, in Bohemia; of Kissingen, in Bavaria; and of Friedrichshall, in Saxe-Meiningen. The quantity of sulphate of soda contained in the pound of water is from 46·5 grains (Friedrichshall) to 123·8 grains (Püllna); that of sulphate of magnesia from 39·5 (Friedrichshall) to 93 (Püllna). The bitter waters of Friedrichshall and Kissingen are distinguished by containing, besides the sulphates just mentioned, a considerable amount of chloride of sodium (61 grains in the pound), by which their action is so advantageously modified that they should be used with preference whenever the use of bitter waters is required. A protracted use of such bitter waters as contain only sulphates, is very apt to impair digestion; they cause a considerable accumulation of fluid in the intestinal canal, and only a very small amount of the salines is absorbed. A proportionate mixture of the sulphates and table-salt seems to prevent any injurious effects following. The Kissingen bitter water differs from that of Friedrichshall merely by containing a certain amount of carbonic acid, whereby it is rendered more palatable and agreeable to the stomach. The physiological effect of the sulphate of magnesia is, on the whole, very similar to that of the sulphate of soda; but the former seems to be more noxious to the system than the latter; thus, after an injection of a solution of sulphate of magnesia into the veins, the heart and the voluntary muscles become paralysed, while, if sulphate of soda be injected, the contractile power of the muscles is in no way diminished.

The physiological effects of the Friedrichshall bitter water have been minutely investigated by Dr. Mosler, with special regard to the weight of the body, animal temperature, the rate of pulsation and respiration, and the condition of the feces and urine. From these experiments it appears that the *purgative* action of the mineral water is more considerable if taken on an empty stomach; but that the *diuretic* effect of the water is greater if taken after a meal.

With regard to the elimination of the solid constituents of the urine, it appears that urea and chloride of sodium are increased, while the quantity of uric acid is diminished, and the increase of sulphuric acid which is perceived is merely due to the quantity of sulphates ingested with the mineral water. The rate of pulsation and respiration is somewhat increased; but animal heat scarcely altered. The weight of the body is reduced, but immediately increased again when the use of the mineral water is discontinued. The waste of tissue induced by it is chiefly confined to the fat. A considerable contrast has thus, by the researches of Drs. Seegen and Mosler, been shown to exist between the Carlsbad and the Friedrichshall mineral waters. Friedrichshall is both a purgative and diuretic, while Carlsbad has only little effect in stimulating the action of the bowel, and may even cause constipation; and, on the other hand, far from being a diuretic, it does not even allow the whole quantity of water ingested to be again eliminated. Friedrichshall increases the elimination of the solid constituents of the urine, and more especially that of urea and chloride of sodium; Carlsbad, on the contrary, diminishes the excretion of urea, uric acid, and chloride of sodium. Friedrichshall, if taken in large doses, diminishes the weight of the body, which is increased by the use of Carlsbad water. As to the mode of using Friedrichshall, I may remark, that it should be drunk before breakfast, if we wish to cause a purgative action; and after breakfast, if we desire to act on the kidneys.

Cases in which it is advisable to administer a mild purgative and diuretic, which is well borne by the stomach and does not weaken the system, are extremely numerous. If there is a considerable accumulation of feces, or congestion towards the head and the lungs, large doses of Friedrichshall, viz., from one to two pints, may be taken at short intervals. In abdominal plethora, especially where it occurs in pregnant women, or when due to sedentary habits, disease of the heart, and indulgence in the pleasures of the table, one tumblerful in the morning, and another in the evening are sufficient. As a diuretic, the water may be given in certain cases of dropsy, and pleuritic and other effusions. Finally, it proves eminently useful in scrofula; especially in swellings of the cervical glands, tumours of the cellular tissue, chronic inflammation of the mucous membranes, and eruptions of the skin.

There are two waters of this class in England which deserve a far more extensive trial at the hands of the medical profession of

this country than has hitherto been given them, as they are, by their chemical composition, admirably suited for the treatment of many cases of disease, and quite equal to the Continental spas of this class. These are the bitter waters of Cherry Rock, near Kingswood, in Gloucestershire, and the Purton Spa, near Swindon, in Wiltshire. The combination of sulphates, chloride of sodium, and carbonic acid, in these waters is peculiarly valuable; and it is a matter of surprise that they have, up to the present time, been so little used. The Purton Spa is in so far superior to that of Cherry Rock as it also contains carbonate of potash, whereby the diuretic effects are increased. In my opinion, little or no value can be attached to the bromine and iodine contained in this water, the quantity of these substances being exceedingly small; but its effects are, no doubt, chiefly due to the sulphates, the chloride of sodium, and the carbonate of potash; while carbonic acid serves to render the water palatable and agreeable to the stomach. I am not aware whether the water of Cherry Rock is at present at all medically employed; that of Purton has, for the last few years, been used in diseases of the skin, certain abdominal affections, and other allied disorders. The two waters mentioned are probably destined to occupy a foremost position amongst all English mineral waters.

5. *Simple Muriated Waters.*—The spas of this class are distinguished by containing a somewhat considerable amount of chloride of sodium as chief ingredient; and the most important springs of this class are the thermals of Wiesbaden, Baden-Baden, and Bourbonne-les-Bains (149° to 155° F.); the tepid waters of Soden, Mondorf, and Canstatt (61° to 77°); and the cold springs of Kissingen, Homburg, and Cheltenham (50° to 53°). The quantity of chloride of sodium contained in the pound of water is from 17 to 114 grains; the Kochbrunnen, of Wiesbaden, contains 52; and the Ragozi, of Kissingen, 44 grains of this substance. These waters, when taken internally, excite the mucous membrane of the stomach, increase the appetite, promote the secretion of gastric juice, and intestinal mucus, and accelerate the desquamation of epithelial cells; the feces are fluidified, and their evacuation is thereby facilitated. The mucous membrane of the respiratory organs is likewise stimulated, and the general metamorphosis of matter considerably altered. After the use of the waters the urine contains more urea than under ordinary circumstances.

Baths of muriated waters have also a considerable influence upon

the system, and which is proportionate to the amount of chloride of sodium contained in the water.

Chloride of sodium is not absorbed by the skin, but it acts as a powerful stimulus to the cutaneous nerves; it promotes peripheral circulation, and may, by reflex action, exercise an important influence upon remote organs, and even upon the centres of the nervous system. The elimination of matter through the skin and the kidneys is, at the same time, increased, especially if motherlye is added to the bath. Brine vapour baths stimulate the mucous membrane of the respiratory organs and promote expectoration.

The physiological effects of the muriated springs are considerably modified by the temperature of the water, which, as I have just remarked, is very different in the several spas of this class. The cold muriated waters of Kissingen and Homburg, contain, besides the chloride of sodium, a very large amount of carbonic acid, by which, if the waters are used internally, the stimulation of the mucous membranes is further augmented; and, if baths are taken, the combined effects of carbonated and muriated water upon the skin are produced. Two of the Kissingen springs also contain a small amount of carbonate of iron, which was formerly believed to be an important constituent of the same; but it is more probable, that it is only the water, the carbonic acid, and the chloride of sodium which are the really active elements of these springs. The mineral waters of Homburg contain a much larger amount of iron than those of Kissingen; and, if the waters are used internally, this ingredient exercises a great influence upon the composition of the blood. The taste of these waters is refreshing, slightly bitter, salty, and astringent. The secretion of saliva is generally diminished by drinking them; but they cause a tough and alkaline mucus to be secreted in the mouth and gullet. Eructations generally follow, and a feeling of warmth is produced in the stomach. Hunger and thirst are increased; the pulse becomes faster, fuller, and harder; and diuresis is augmented. During the use of Homburg water the feces assume a dark colour; sulphuret of iron being formed in the bowel. Concerning the effect of the waters upon the intestines, they at first not unfrequently produce costiveness; more especially the Homburg water. The effect of the Kissingen springs upon the bowels is more considerable if it is allowed to stand for some time, as great part of the carbonic acid then escapes. The physiological effects increase in proportion to the quantity of water taken, and to the time its use is continued.

The thermals of Wiesbaden taste like chicken broth highly salted; they increase the flow of saliva and buccal mucus, induce frequent deglutition and acuteness of taste, and cause eructations, increase of appetite, and warmth in the epigastrium. One or two hours after drinking even small quantities of the water the flow of urine is increased, and it contains larger quantities of urea, uric acid, and chloride of sodium than is the case if corresponding quantities of ordinary water are drunk. The absorption of the chloride of sodium is facilitated by the high temperature of the water, and the action of the bowel is therefore rather diminished than increased, especially if only small doses are taken. Larger quantities have a more considerable effect upon the bowels; they augment the secretions of the skin and all the mucous membranes; menstruation becomes more abundant, and ensues earlier than otherwise; the pulse is accelerated, and the weight of the body sometimes diminished. If the water has been used for six or eight weeks in succession symptoms come on which indicate saturation of the system. Great aversion to the water is then experienced; the tongue becomes furred; thirst is troublesome; and there is a feeling of heaviness in the stomach, with other gastric symptoms. These are signs that the use of the water should be discontinued; or vomiting, diarrhea, and congestions towards the head and chest would follow.

The effects of the waters of Baden-Baden and Bourbonne-les-Bains upon the metamorphosis of matter, are only imperfectly known. The former spa is more a resort for healthy persons in search of pleasure and relaxation than a refuge for patients; beside which, the water of that place, if used for medical purposes, is scarcely ever administered in its natural state. The chief action of the water, when taken as it is, seems to be, to induce constipation, which is probably due to its high temperature, and the use of aperient medicines therefore become necessary. The physicians at Baden-Baden are in the habit of adding to the water Carlsbad salt, bicarbonate of soda, iodide of potassium, lactate of iron, and other medicines. The thermal water used for bathing is generally mixed with Kreutznach motherlye.

The muriated waters are chiefly efficacious in gout, rheumatism, scrofula, and abdominal plethora. They act by exciting the secretions of the intestines, kidneys, and the skin, and thereby reducing the habitual plethora, eliminating the gouty poison, and promoting a healthy metamorphosis of matter. The Wiesbaden treatment is

very successful in chronic rheumatism, tic-douloureux, and sciatica. In dyspepsia which is owing to chronic catarrh of the stomach, liver disease, abuse of intoxicating liquors, or deficient secretion of gastric juice, arising from sedentary habits and other causes, the internal use of the Kissingen Ragozi should be combined with warm brine baths. In chronic diarrhea, arising from intestinal catarrh, the Kochbrunnen, of Wiesbaden, is the best water of this class that can be chosen. In abdominal plethora, in weak, irritable, and scrofulous patients, to whom the use of the bitter waters and of Marienbad might prove detrimental, the Ragozi or the Homburg Elisabethbrunnen should be prescribed. If judiciously administered, these waters cause neither constipation nor relaxation of the bowels. The thermals of Wiesbaden are appropriate in the milder forms of abdominal plethora, where constipation, hemorrhoids, and hyperemia of the liver are troublesome; but, in severe forms of this disease Carlsbad is preferable. If catarrh of the bronchial tubes is at the same time present, we may order the waters of Soden or those of Canstatt, which contain a rather considerable amount of sulphates, whereby both the effects of the bitter waters and the muriated springs may be produced. Amongst the English spas of this class Cheltenham is the one most closely allied to Canstatt; but, as the Cheltenham water contains very little carbonic acid, it is inferior to the latter.

Muriated waters often prove curative in catarrh of the larynx and the lungs; those containing a moderate quantity of solids being generally better suited for cases of this kind than the strong muriated waters. They have also been warmly recommended for consumptive patients. They cannot cure phthisis; but if patients affected with this disease suffer, at the same time, from troublesome catarrh, indigestion, and disturbed nutrition, several springs of this class may be taken with great advantage, for allaying the irritation of the respiratory organs, for promoting digestion, and giving tone to the system generally. A prolonged stay at Soden is very beneficial, as the climate there is exceedingly temperate and not subject to sudden changes. The tranquillity of this place contrasts favourably with the noise and bustle of Baden, Wiesbaden, and Homburg, which spas are centres of gambling and fashion, and on this account frequently prove ruinous to patients unable to bear excitement.

Muriated springs are of great therapeutical value in certain diseases of the liver, such as hyperemia of that organ, when

arising from want of exercise, too generous diet, and accumulation of fat; icterus due to catarrh of the mucous membrane of the duodenum, and simple induration and enlargement of the liver. Gall-stones are, under the influence of the same treatment, discharged, probably in consequence of the augmented secretion of bile washing out the excretory ducts of the liver. The waters are also successful in tumour of the spleen arising from ague, suppressed catamenia, or hemorrhoids.

Chronic diseases of the skin connected with abdominal plethora, arthritic ulcers, and old gun-shot wounds, are often cured at Wiesbaden and Bourbonne-les-Bains, even if projectiles and pieces of necrotic bone are still present. These latter often pass away after the baths have been taken for some time, after which the ulcers heal. The waters should, however, only be prescribed some time after the accident, and when the inflammation has subsided; for, if they are used in too early stages, the result may be unfavourable. They must also be avoided by persons having suffered from fractures, and where union has only recently taken place. Whether the waters have a tendency to soften fibro-cartilaginous and osseous tissue, as has been asserted, is by no means settled; but, it is a fact that recently united bones often fracture again if these waters are used.

6. *Muriated Lithia Waters.*—Small quantities of lithium are probably contained in all mineral waters which are rich in chloride of sodium; but, until now, we know of only two springs which contain so much lithium that this may be said to be the characteristic feature of the water. These are the Murquelle and the Fettquelle, of Baden-Baden, in the former of which Professor Bunsen has discovered 2·4 grains, and in the latter 0·2 grains of chloride of lithium in the pound of water. They have only within the last two years been medically employed, chiefly in cases of gout and lithiasis; and there is, consequently, as yet no sufficient experience concerning their therapeutical powers; but, from what we know about the pharmaceutical properties of lithia, they may be safely recommended in cases of uric acid diathesis connected with gravel, and in cases of chronic gout. The experience already made at Baden-Baden with these waters is, on the whole, very favourable.

7. *Brines.*—The brines contain a very large amount of chloride of sodium which is extracted from them for commercial purposes.

Those of Rehme, or Oeynhausen, in Westphalia, and of Nauheim, in Electoral Hesse, have the greatest reputation as therapeutical agents. The chief spring of Rehme contains 256, and that of Nauheim 181 grains of table salt in the pound of water. Brines are only seldom internally administered—and if so used, they are diluted with fresh water. Brine baths, on the contrary, are most extensively employed, and their action upon the system has lately been investigated by several observers. From these experiments it appears that the primary effects of the brine baths are only due to their influence upon the cutaneous nerves, for neither chloride of sodium nor water is absorbed by the skin. The chief effects are increase of the waste of tissue and of diaphoresis; at the same time the nervous and muscular system are stirred up, the appetite is increased, the bowels are more easily voided, the abdomen and the chest become expanded, the pulse rises, the sexual desire becomes stronger, and the mind more settled and cheerful. But if the use of the baths is continued too long, febrile symptoms, emaciation, pain in the back, &c., are apt to follow.

Brines do much good in diseases caused by impaired nutrition and innervation, such as scrofula and anemia, and in complaints in which excitation of the skin is desirable, as, for instance, in rheumatism, certain forms of paralysis, and catarrh of the mucous membranes. In scrofula they are most effective where there are no considerable local affections, and in children; while in adults, and where there are glandular tumours and scrofulous diseases of the skin, little or no benefit is to be expected. Scrofulous affections of the mucous membranes, such as subacute and chronic bronchitis, ozena, otitis externa, and catarrh of the vagina, are amenable to the treatment. The remedial powers of brines are often strikingly displayed in scrofulous diseases of the bones, such as caries, necrosis, osteo-porosis, &c., even if the constitution has already given way, and hectic fever should be present.

Brines are not advisable for patients suffering from phthisis; and Rehme should be particularly avoided by them, as the air of that place is too bracing. Patients affected with secondary syphilis have also sometimes been sent there. They experience no benefit whatever from the treatment; but, on the contrary, become so much worse that in cases where the diagnosis is doubtful, these baths may be advised as a valuable means for recognizing the nature of the disease.

Brine baths are often said to be apt to produce congestion to the

head, and apoplexy; but this is by no means the case provided they are judiciously used. In cases of paralysis after typhoid and other fevers, or after parturition, they frequently do a great deal of good. In these affections, and in hysterical, rheumatic, and reflex paralysis, as well as in lead palsy, their use should be combined with Faradization of the suffering parts. They produce absorption of rheumatic and gouty effusions, and may arrest the further growth of fibrous tumour of the uterus. They can be strongly recommended for decrepidity in old persons, and in such as have become prematurely old; they also have a favourable influence upon the evolution of persons who have just attained the period of puberty, and upon that of the fetus.

8. *Iodo-Bromated Muriated Springs.*—These waters, the most important amongst which are those of Kreuznach, Hall, Dürckheim, Wildegg, and Castrocaro, contain chiefly chloride of sodium (from 38 to 404), iodide of sodium (0·02 to 0·96), and bromide of magnesium (0·01 to 0·51). Most of them are cold; the only iodated thermals in Europe being those of Montecatini, in Tuscany (92° F.); and of Lipsick, in Sclavonia (115° F.).

Very little is at present known concerning the effects of the iodo-bromated muriated springs upon the general metamorphosis of matter. The action of such waters depends chiefly upon the proportion of iodine and bromine to the quantity of other salines found in them. There are some springs, as, for instance, those of Castrocaro, which contain a comparatively large amount of iodide of sodium, and bromide of magnesium; but, at the same time, so much chloride of sodium that they must be considerably diluted before they can be used internally, whereby the production of the specific effects of iodine and bromine is retarded; on the other hand, such waters as contain comparatively little iodine and bromine, as well as only little table salt, and which can therefore be used as they are, will more readily cause the peculiar effects of the two former substances. If baths of these waters are taken, neither iodine nor bromine is absorbed by the skin. Taken internally, the waters promote the secretion of saliva and the appetite, and stimulate the mucous membranes generally. If they, at the same time, contain a certain amount of sulphate of soda, the bowels are likewise acted upon; and large quantities promote diuresis. A too prolonged use of them may produce the well known symptoms of iodism.

As therapeutical agents, these waters are chiefly successful in

scrofulous infiltrations of the glands, where brines fail to afford relief; also in scrofulous diseases of the skin, particularly lupus, sycosis, lichen, and scrofulous ulcers; and even in caries and necrosis they not unfrequently prove curative. While brines are more beneficial in cases of short duration, and in children, the waters we are now treating of are chiefly effective in cases of long standing and in scrofulous affections of adults.

They are likewise to be recommended in chronic metritis and oophoritis, with consequent anomalies and swelling of the ovaries; and in enlargement of the prostate and the testicle, when this is due to inflammation. In fibroid tumour of the uterus the waters act, according to Scanzoni, chiefly by reducing the size of the womb, after which the troublesome symptoms which are owing to the enlargement of the organ, such as hemorrhage, uterine colics, &c., disappear. In tumours of the ovaries, Scanzoni does not advise the use of the waters if the tumour is of a carcinomatous character, and so long as there are symptoms of congestion; but in other tumours of this organ their further growth may be arrested or retarded. The waters are also efficacious in tertiary syphilis, more especially in patients who have taken much mercury, and in whom syphilis is associated with scrofula.

The waters of Hall, in Austria Proper, enjoy a special reputation in cases of bronchocele; and it is generally known as Kropfwasser. It is true, that in lymphatic bronchocele where the thyroid body is infiltrated with colloid matter, favourable results may be expected from this spa; but in other forms of this disease the Kropf-wasser does not justify its name.

9. *Earthy Springs.*—The chief contents of these waters are sulphate and carbonate of lime, to which is added a certain amount of chloride of calcium, carbonic acid, and chloride of sodium; and the most important earthy springs are those of Wildungen, Leuk, Lippspringe, Weissenburg, Bath, Lucca, and Pisa. The quantity of carbonate of lime is from 2 to 10·2; that of sulphate of lime from 0·5 to 17·2.

Most springs of this kind are thermals, their temperature varying from 81 to 123. No exact researches have as yet been made concerning the physiological effects of earthy springs. If drunk, a feeling of warmth is produced in the stomach, the appetite is increased, and digestion promoted. Constipation sometimes follows the use of small doses, while large quantities, on the contrary,

cause diarrhea. In some persons even small doses of these waters produce fulness in the head, sleepiness, vertigo, and pressure in the epigastrium. The excretion of the kidneys and the skin is generally increased. The water being very hard, baths of it cause a rather unpleasant sensation in the skin. Their action seems to be, on the whole, exciting and astringent. It is, however, very different according to the temperature of, and to the length of time the patient remains in, the bath. If they do so for a considerable time, such as five hours and more every day, for three or more weeks consecutively, as is the custom at Leuk, Schintznach, and other Swiss spas, a peculiar eruption appears on the skin which sometimes resembles herpes; in other cases erythema, or the scarlet rash, or small thickly-studded papulæ are formed.

The Wildungen water is distinguished from the other spas of this class by containing a very considerable amount of free carbonic acid by which its effects are much modified. It has a special and well deserved reputation for its curative powers in gravel and lithic diathesis. It is no solvent of renal calculi, nor of stone of the bladder; but it is a capital diuretic, and not only promotes the elimination of gravel and renal calculi, but, by its tonic action on the mucous membrane of the urinary passages, seems to prevent the formation of fresh concretions. It is also much used for chronic catarrh of the bladder, neuralgia of the urethra and the neck of the bladder, dysuria, ischuria, and incontinence of the urine.

The waters of Leuk are mostly employed in atonic ulcers and in chronic diseases of the skin, such as eczema and psoriasis. Bathing is of chief importance in the thermal establishment of that place; and there can be no doubt, that very obstinate cases of skin disease in persons of torpid constitution may be improved or cured by the Leuk treatment, a modification of which, according to the principles of modern medicine is, however, to be strongly recommended to the physicians of that spa. The baths are also much used for gouty and rheumatic effusions, and the functional disturbances caused thereby, especially in patients of impaired nutrition and diminished activity of the skin.

The spas of Lippspringe, in Westphalia, and of Weissenburg, in the Canton of Berne, are of much repute as remedies for diseases of the respiratory organs. At Lippspringe it is chiefly the nitrogen ascending from the springs, and which is inhaled by the patients, to which the beneficial effects of the treatment are ascribed, while the physicians of Weissenburg insist upon the lime being the cause of

the curative effects obtained. It seems that in the incipient stage of consumption, where there is much irritation and tendency to inflammation, together with bloody expectoration, much improvement may result from the use of these spas. Time will show whether they are really successful in the latter stages of this disease.

Bath, Pisa, and Lucca, have often proved successful in gout and rheumatism, certain forms of paralysis, atony of the stomach and intestines, and diseases of the skin.

10. *Indifferent Thermal Waters.*—These contain only a few grains of salines (from 2 to 4), and are of a high temperature. The water is limpid, colourless, or slightly blue, devoid of odour and of any physical peculiarities different from those of ordinary warm water. The temperature in the several spas of this class, the most important of which are Gastein and Wildbad, is from 81° (Landeck) to 120° (Teplitz). The Wildbad water has exactly the temperature of the blood. All these waters, but especially those which have a somewhat higher temperature, possess very remarkable physiological and therapeutical properties. Baths of them promote the action of the skin, and excite the peripheral nerves; and, by reflex action, the nervous centres are affected, especially if they are weakened. Effects of the latter kind are chiefly caused by such thermals as issue at high altitudes; and it is probable that the diluted air and the diminished pressure which exists in those regions essentially contribute to the effects of the waters.

The waters of Gastein are chiefly used for bathing. These baths, which are taken in tubs of stone, are not exciting in the ordinary meaning of the term; for persons with habitus apoplecticus, heart disease, and even hemiplegic patients shortly after a seizure, often bathe in this water without any unpleasant consequences, but are, on the contrary, improved by it. The pulse is only accelerated if the water is very hot; but if the temperature is somewhat cooler the pulse is retarded Gastein is mostly resorted to by decrepit, old, and paralytic persons, who are, by the use of the baths, often restored to comparative health and vigour. The average duration of the baths is from a quarter of an hour to an hour, and even more; and that of the treatment, from four to six weeks.

The baths of Wildbad, in Wurtemberg, are chiefly taken in piscines, or swimming baths. These are large reservoirs the soil of which is covered with fine sand, through which the water rises from

the depth at a temperature just suitable for bathing. It is therefore not necessary to heat or cool the water, and a constant renewal of it is also rendered easy. Bubbles of nitrogen, in which the Wildbad water is very rich, continually glide along the surface of the body, and produce a sort of titillation which is by no means unpleasant. If the stay in the bath is too prolonged, weariness, fatigue, headache, and febrile symptoms are apt to follow. The baths prove most beneficial in the several forms of rheumatism and morbid affections consequent upon it, even when of a severe kind and of long standing.

The springs of Pfäffers, in Switzerland, are used for drinking as well as for bathing. This water agrees very well with the stomach even of weak persons. The elimination of urine and the perspiration are increased by its use; but it has no action on the bowels. The baths have different effects according to their temperature and duration. The usual dose for drinking is from four to eight tumblerfuls. The climate and neighbourhood of Pfäffers are so gloomy and wild, that patients who require more exhilarating and genial surroundings had better not be sent to that spa. In this respect Ragatz is more pleasant; but, as a large number of tourists are continually on the move at this latter place, it is not suited for patients to whom quiet is necessary.

The waters of Teplitz, in Bohemia, are of great therapeutical value in all forms of paralysis and hyperesthesia in which there are no such structural diseases as would preclude the possibility of a cure. Besides the thermal baths, the douche, moor-baths, and moor-cataplasms, are used there. The Hauptquelle of this place has a temperature of 120°.

The water of Schlangenbad, in Nassau, is almost exclusively used for bathing. It has a soft velvety feel if taken between the fingers, and imparts a most pleasurable sensation to the skin. Baths of this water render the skin exceedingly soft, supple, and delicate, and are therefore extensively used by ladies, especially by those in a somewhat advanced age. They also do good in irritability of the nervous system, especially if due to, and connected with, disturbances of the menstrual function, neuralgic dysmenorrhea, &c.

The springs of Warmbrum, in Silesia, and of Tüffer, in Styria, are applicable in all cases for which the other indifferent thermals are used. Those of Clifton, Buxton, Landeck, and Plombières, are, on account of their lower temperature, suitable for the milder forms

of those diseases in which Gastein and the other more powerful spas of this class are chiefly employed.

11. *Chalybeates.*—In chalybeate springs, bicarbonate of protoxide of iron is the principal ingredient; and this is generally accompanied by carbonic acid and bicarbonate of manganese, by which the effects of the iron are much modified. The most powerful chalybeates are those in which, with the exception of iron, only a small amount of solid ingredients and much carbonic acid is contained. The quantity of iron found in the true chalybeates varies from 0·25 to 0·8 grains in sixteen ounces of water; springs containing less iron than 0·2 are no chalybeates. True chalybeates are generally limpid and devoid of smell; the taste is that of carbonated water slightly mixed with that of iron. The physiological action of carbonic acid is, in many respects, similar to that of iron. By small quantities of both digestion is promoted, the activity of the heart, and muscular and nervous power strengthened. The acidulous chalybeates are most admirable remedies for anemia and all morbid conditions consequent upon it, such as certain forms of indigestion, constipation and diarrhea, amenorrhea, neuralgic dysmenorrhea, blennorrhea of the uterus and the vagina, sterility, and tendency to abortion. In certain diseases of the nervous system, in headache, neuralgia in the back, chorea, tremor, paralysis, hypochondriasis, and hysteria, the spas of this class also prove beneficial. The waters of Schwalbach, in Nassau, Spaa, in Belgium, and Pyrmont, in the Principality of Waldeck, enjoy the greatest reputation for their curative powers. Those of St. Moritz, in the upper Engadin will, no doubt, shortly rival in fame its fellows of more ancient renown.

A second class of chalybeate springs are those called saline acidulous chalybeates, which, besides bicarbonate of iron and carbonic acid, also contain a certain quantity of carbonate and sulphate of soda, and chloride of sodium. They act beneficially in most diseases in which the alkaline acidulous waters, the alkaline salines, and the muriated waters are employed, especially if a certain degree of anemia is associated with those complaints. In many cases it is difficult to decide whether a true or a saline acidulous chalybeate should be prescribed; but, it may be laid down, as a general rule, that in pure anemia the former, and in anemia complicated with abdominal disturbances, the latter are preferable. The most important spa of this group is Franzensbad, in

Bohemia, where several saline chalybeates of unrivalled composition rise from mica-slate. The amount of iron contained in the principal spring of this place is 0·37, that of sulphate of soda 25·2, that of carbonate of soda 4·3, that of chloride of sodium 9·3 grains, and that of carbonic acid 4·5 cubic inches in the pound of water. The iron mineral moor which is employed for baths and cataplasms, has a most invigorating influence upon the system. Moor-baths are chiefly used in certain forms of paralysis, gout, and rheumatism, in patients who are below par.

12. *Sulphurous Waters.*—The physiological effects of the sulphurous springs, the chief contents of which are sulphureted hydrogen or sulphurets of metals, are widely different according to their temperature, and the other solid and gaseous ingredients contained in them. Sulphurous thermal springs considerably accelerate circulation. The waters of Eaux Bonnes, when taken internally, produce agitation, sleeplessness, and general excitement of the system, the same as is caused by drinking strong coffee. The power of the muscles appears increased, the complexion becomes florid, a sensation of heat in the pharynx, and pains behind the sternum and between the shoulders are felt. Sometimes there is increased appetite, pain in the abdomen, and constipation.

The waters of Saint Sauveur, if used for bathing, have a sedative effect on the system. They contain a large quantity of Barègine which imparts an oily feel to the water. On the other hand, the springs of Barèges are very exciting. They augment all secretions, and after a few days use, produce febrile symptoms.

Exact physiological researches on the effects of the cold and thermal sulphurous springs are almost entirely wanting. The only observer who has made a few experiments on this subject is Dr. Böcker, who was led to the conclusion that after the use of such waters, the exhalation of carbonic acid by the lungs, and the elimination of uric acid by the kidneys were increased.

The therapeutical action of the sulphurous spas is due to the sulphur, the water, its temperature, and the salines contained in it. They are extensively used in diseases of the skin, and prove particularly beneficial not only in cases of acne, but also in the milder forms of pityriasis, psoriasis, prurigo, and sycosis. In a variety of cases where effusions are to be absorbed, as in swellings of the joints, in caries, and necrosis, in old gun-shot wounds, and in gout and rheumatism, they are used with advantage, provided all inflammatory

symptoms have subsided. In ozena, clergymen's sore-throat, chronic catarrh of the larynx and the bronchial tubes, they frequently afford relief. In chronic poisoning by mercury, lead, and copper, they favour the elimination of the poison and promote recovery. They have been much praised as remedies for abdominal plethora, and more especially for hemorrhoids; it seems, however, that they only do good in such cases if, besides the sulphur, a certain amount of salines is contained in them, or if baths of a high temperature are given. Finally, they may serve as means of diagnosis in cases of secondary syphilis. Under their use syphilitic affections of the most various kinds are very much aggravated; and if no visible symptoms of the distemper exist, but this is latent in the blood, it frequently manifests itself by specific affections of the mucous membranes and other parts, after the sulphurous spas have been used for a week or fortnight. The sulphurous waters are employed externally and internally, and mineral mud-baths form a valuable auxiliary to this treatment.

The most important sulphurous thermals are those of Aix-la-Chapelle; the principal spring there has a temperature of 131°, and contains 0·07 grains of sulphuret of sodium, and 20 grains of chloride of sodium. The waters of Baden, near Vienna, are, on account of their lower temperature (from 89° to 95°), inferior to those of Aix-la-Chapelle, especially for cases of rheumatism and rheumatic paralysis; but, as the air in Baden is much more bracing than at Aix, the former spa is more appropriate for catarrh and scrofula. Amongst the Pyrenean sulphurous baths, those of Bagnères de Luchon are the most important, and prove useful in most complaints in which Aix and Baden are generally prescribed.

Art. VI.—*Contagion Viewed Practically.* By D. C. O'Connor, A.B., M.B., T.C.D., L.R.C S.I., Professor of Practice of Physic, Queen's College, Cork.

A subject viewed scientifically becomes a widely different thing from the same subject when viewed practically. In the former manner we ascertain the truth of a general law, or a fact. In the latter we view the same things in their relation to ourselves or our interests. For instance, nobody doubts, that during a thunder storm, he may be killed by lightning. Still, this occurrence is so rare

that practically it excites no fears, except in the minds of the most timid. From time to time some one dies of hydrophobia; nevertheless, with the exception of mayors in the dog days, no one expects an early death from this cause. In like manner, it is a fact scientifically established, that the persons of those affected with certain diseases produce a poison capable of transmitting the same disease to others. But again the questions of practical importance to determine are:—What are these diseases in which this property resides? What is the amount of danger in such cases? I believe that on both these subjects much error or exaggeration is to be found in some medical writings—amongst many members of the profession, and still more amongst the general public. It is with the view of discussing it rather than with the vain hope that any reasoning of mine would lead to determinate results in points of so much difficulty, that I now bring the subject under notice. The difficulty which is inherent, and will for ever attach, to this subject, arises from the fact, that there is no disease contagious which is not, at the same time, epidemic. To the latter influence every person is alike exposed; and, in any given case, it is not possible to assert that it was not from this source, rather than from contagion, the poison was imbibed. If a person were found dead in the streets of a town which was being shelled by an enemy, we would not infer, without very positive proofs, that he came by his death through private malice. Neither could we bring home a charge of theft against a person merely because he had a bad character, if there were several others in the house with him at the time the property was abstracted. The same difficulty exists in the proof of every case of supposed contagion, namely, that it might be explained by another cause equally potent, and to which the patient was as much exposed. It must be admitted, however, that sufficient proof, grounded on the highest probability, has been afforded, that some diseases are contagious; and assuming such to be the fact, I shall confine my observations to an inquiry into the manner of their propagation, and whether some diseases might not be removed from this category which are generally included in it.

Some authors classify as contagious the idiopathic fevers both of this climate and the tropics—the plague, cholera, influenza, dysentery, erysipelas, ophthalmia, gangrene, and a host of others. Whilst some, again, limit the number to the exanthemata and typhus fever. This diversity of opinion would show that the proof cannot be very positive in many of these diseases as it does not coerce the

judgment of men of ability with both power and opportunity for observation.

Admitting the correctness of the second classification, let us inquire into the arguments which are adduced to prove the existence of contagion in the diseases not included in it. After much discussion and much human misery from quarantine laws, it is practically settled, that yellow fever is an epidemic, not a contagious disease. The same might be said of the plague, Asiatic cholera, influenza. Opinion is not so settled with reference to erysipelas, ophthalmia, dysentery, gangrene, and puerperal fever, about which a few words may be said before entering on the other branch of the subject.

The proof of contagion in all these diseases is derived from the fact of several cases appearing, at the same time, in hospitals, jails, workhouses, the holds of ships, and other ill-ventilated places. It is never asserted that in most of these diseases contagion is to be dreaded from an isolated case—puerperal fever being the exception—which, by some, is asserted to be most virulently contagious. Still, the question in all these cases might be asked:—May not the spread of these diseases in the places referred to be due to epidemic influence existing in the atmosphere, and intensified by the bad air of the locality. These diseases are never found to spread in an hospital unless when they exist in a less obvious form amongst the general community; and it might be readily asserted that the hospital and workhouse in which they spread must be over-crowded and ill-ventilated. It is only such circumstances reveal to us that most hospitals, notwithstanding the utmost care, are not conducive to the general health of the inmates, apart from the medical treatment these receive.

We do not know what is the change in the atmosphere which, under the same exciting cause, will produce one form of disease at one time, and a different form at another; but we can readily see that where the atmosphere is most stagnant, equally in the lanes and alleys of the poor as in hospitals, this condition will exist in the highest degree. At the present time I am in attendance on a male and a female orphan asylum, entirely separated, and without any communication existing between them. Still the same form of light catarrhal ophthalmia exists in each. If the child be removed from the poisoned atmosphere, the disease quickly disappears, but returns on his or her re-admission. On inquiry, I find the same disease is prevalent through all the poorer districts of the city. In every form of epidemic disease—those never asserted to be contagious

(I may instance the mumps)—several persons resident in the same house become simultaneously affected, while the inhabitants of adjoining houses remained entirely exempt. In the outbreak of Asiatic cholera in the Cork Workhouse its ravages were confined to the male wards several weeks before it appeared amongst the female inmates—showing, that while the poison exists in all parts of the atmosphere, it might assume increased power of infection in some localities.

Of all epidemic diseases—the contagious character of which is doubtful—none has excited so much interest as puerperal fever, from the belief entertained by many, that the physician, whose duty it is to cure, often carries to his patient a most malignant disease. And still the evidence afforded of this fact is such as would not stand for one moment in the ordinary affairs of the world. If a man were sent to trial for manslaughter, on similar testimony, the question of his guilt would not be entertained for one moment; and why should old women's stories, told by timid physicians, be accepted as arguments by the medical profession? In this city we recently had, unfortunately, ample opportunities of witnessing the development and spread of this disease, and during the whole time no one instance has been brought forward which would support the opinion that it was contagious. It has been remarked, during the prevalence of this disease amongst us, that erysipelas also prevailed extensively; and it will be in the recollection of the members of the Cork Medical and Surgical Society that several papers were read, at the same time, on diffuse cellular inflammation and phlebitis, as a consequence of wounds or bruises. Is not the condition of a puerperal female precisely analagous to that of a person who has received a bruise or hurt? The tenderness and pain which invariably exists for some days in the uterus, even in the most favourable cases, shows that such violent muscular exertion and strain cannot have taken place without some injury. If a man has got a slight wound or bruise, or even a blast of cold air during the prevalence of erysipelas, we are not surprised to find this disease has seized on the injured part, and changed into its own character an inflammation which would otherwise be innocuous. Why should an injured uterus be exempt from similar consequences? It will be said there is often but little local pain in puerperal fever, and that there is sometimes no trace, after death, of disease in the uterus. Both these cases I believe to be exceptionable; still they do not prove that inflammation did not exist during life, as every

day's experience shows us that fatal disease might exist in internal organs without pain, and erysipelas in external parts might produce such a fever as would prove fatal, without leaving any trace of the original affection on a *post mortem* examination. We can, then, readily understand that an erysipelatous inflammation of the uterus may kill, without the spreading of the disease to the peritoneum—which, however, in the vast majority of cases, becomes ultimately engaged. Supposing, then, this disease to be an inflammation, peculiar in its character from the nature of the organ affected, and the condition of the patient at the time, it would require very strong proofs to establish its contagious character; still more to establish the truth of such anomalies as the following:—That the patient affected with puerperal fever produces a poison capable of being communicated to other puerperal females, and to no others, male or female; and that the puerperal patient is free from this danger, except for a period of about one week after her confinement! In the whole range of medical science there does not appear to be anything similar to this. Human beings imbibe with impunity atmospheric poisons which destroy cattle, and *vice versa*—but no individual of our species is affected with any disease to which every other is not more or less liable, making allowance for difference of sex and race. There is not on record a more striking illustration of the point I am now inquiring into, than the prevalence, at one time, of trismus infantum in the Dublin Lying-in Hospital, and its subsequent disappearance on a more perfect ventilation of the wards. But for this latter fact, the disease would be classed with puerperal fever amongst the contagious diseases.

Having thus briefly considered the question of contagion in diseases in which any doubt exists, let us inquire into the mode of its propagation in diseases admittedly contagious, namely, the exanthemata, typhus, and typhoid fevers. During the prevalence of these diseases there are two sources of infection, namely, the poison existing in the atmosphere, and an emanation from the body of the person affected. In addition to exposure to this poison, the individuals must be predisposed to its reception; of which some striking instances have come under my notice.

Prior to the appearance of Asiatic cholera in the Cork workhouse, in 1849, I was sending to the fever hospital a daily average of five or six patients from the male wards; as soon as the new epidemic appeared, not a single case of fever presented itself any longer amongst the male inmates, although they continued to arise in

the female wards for nearly a fortnight afterwards, when a similar result followed the outbreak of cholera in that part of the house. It is evident that the victims of cholera were those who previously would have been attacked with typhus fever, from the same predisposing cause. Though it is not always possible to assign a reason why one individual will be attacked by the poison when another equally exposed escapes. Still some striking instances have come under my notice which show that this is sometimes ascertained and remedied. At the period just referred to, it was found that the men engaged in burying the dead fell victims to cholera in great numbers; and at the time it was believed to be occasioned by contagion, arising from their close proximity to the bodies of those who had died of the disease. However, it was suggested that the burials should no longer take place at night, and that those engaged in that office should be very well fed and clothed; and from that time forward these men enjoyed almost an immunity from the disease. The same might be said of hospital attendants, and from the same cause, that they were thoroughly well fed.

If we compare the two sources of contagion to which I have referred, and examine facts, the second will, I think, almost practically disappear as a means of propagating disease. If we could, with justice, diminish the terror which exists in the public mind on this subject, we would prevent much misery, and remove a source of disease to which no cause so strongly predisposes as fright. Some, if they meet the fever-chair in the street, or converse with some one who has had intercourse with a patient in fever, or by accident find themselves in the neighbourhood of a house where the disease exists, go home in great misery, and perhaps fall a victim to their own terrors. The writings of some physicians and the example of others, give grounds for these apprehensions amongst the ignorant public. Until recently the physicians attached to the fever hospital in this city were prohibited from practising midwifery, lest they should contaminate their patients; and during the late epidemic of puerperal fever some physicians refused to attend in such cases, even as consulting physicians, lest they should carry the disease to their own patients. It was amusing to witness the ablutions practised by some of the older physicians after attending a case of fever. One that I knew took off his coat, mixed some spirits and water, with which he washed his face, hands, and arms. Lady Macbeth washing out "the damned spot" had not a more solemn and tragic countenance.

The grounds on which physicians entertain such a dread of contagion from personal intercourse with the sick rests on the fact that dispensary medical officers, students, and clergymen suffer from the disease in a larger proportion than any other class. But it has always appeared to me, that it was not from the patient the disease was acquired in such cases, but from the atmosphere in which he resided, and from which he himself had imbibed the poison. In the cabins of the poor, every inmate is attacked nearly simultaneously, even though the first assailed should be removed to a fever hospital. When I was physician to the workhouse I invariably remarked that a few cases of contagious disease introduced from town, never communicated the disease to the other inmates, but that at some subsequent period the atmosphere of the place became, as it were, infected in some unknown manner, and then large numbers continued to be assailed, till the disease exhausted itself, which it did long before it had reached all those who were liable to it.

The following illustrative fact came under my notice:—During the winter 1846–7, down to the end of April, a most dreadful fever raged in the Cork workhouse. No official escaped the disease except one chaplain and myself. Nevertheless, I wrote to an insurance company in the month of May as follows:—"If I wished to escape all danger from fever I would go to reside in the workhouse," so completely had the fever left it, though it still raged among the more respectable inhabitants of the city, and although a new population was constantly arriving at the workhouse, not previously exposed to its contagion. If it were solely or principally from the person fever spread it ought to increase in a sort of arithmetical progression in fever hospitals and workhouses, till they became pestilential. Still there is much less danger in visiting the former than attending a solitary patient in an infected house or district inhabited by the poor.

The following fact will show how short a distance contagious miasma is capable of spreading when brought into contact with pure air:—At the period already referred to, a quadrangular building was partly occupied as an auxiliary workhouse and fever hospital—one side being in the possession of the governors of the lunatic asylum. From all sides windows opened into a common yard. Fever, of the most dreadful malignancy, broke out amongst the workhouse inmates; whereas, not a single case occurred amongst the lunatics, because they were not crowded and were well fed.

During the whole of my experience, extending now over a period of thirty years, I have never met a case where fever spread from one member of a family to another in a respectable house, although, we know how close affection brings them together, as attendants, or in their dying moments, or in convalescence. I have made inquiries amongst the physicians of greatest experience in this city, some dead and some still living, and find that their experience corresponded with my own. Nevertheless, we find many, even physicians, who assert that not only may you imbibe disease at the sick bed, but carry a stock of it with you to all your patients and friends. In this city we practice every branch of the profession, "omnia possumus omnes." We pass from the fever to the midwifery case, and still it has never entered into any one's head to think that fever has been communicated in this manner to his puerperal patient. In the recent outbreak of scarlatina (the most contagious of all diseases), not a single instance occurred where a puerperal patient was attacked with the disease, unless where other inmates of the same family were ill at the same time, or previously.

If disease were communicable in this way, the families of physicians must suffer more than others; still, in my own instance, though living in an atmosphere of scarlatina, none of my children acquired that disease; and on inquiring of my professional friends through the city, I find their families have had nearly a similar exemption. I know stories are told of a physician having had several of his patients attacked with puerperal fever, and argued from this that he had communicated the disease to them; and stories are told of a trunk having been opened, and of those in its vicinity being attacked with fever. The greatest errors under which mankind have laboured at all times arose from placing in the relation of cause and effect, facts that were only coincident or consecutive. The argument "post hoc ergo propter hoc," furnished abundant evidence to sustain for centuries a belief in astrology, to cause witches to be drowned, to make palmistry a science, and dreams prophetic; and, until lately, to cause letters from the tropics to be roasted and perforated, in order to kill or let out the poison. But medical logic should be much more strict for the sake of the character of its professors, and for the great interests involved in its conclusions.

I have thus endeavoured to show that many diseases supposed to be contagious are but localised epidemics; that contagion from the person affected has but the smallest possible influence in producing

disease, when the surrounding atmosphere is not impure, and that, consequently, its transmission by those parties is practically impossible.

ART. VII.—*Some Observations on the Administration of the Vapour of Chloroform in Obstetric Practice.* By EDWARD B. SINCLAIR, A.M., M.D., T.C.D., Vice-President of the King and Queen's College of Physicians in Ireland.

THE practice of inducing anesthesia by means of the vapour of chloroform, in cases where surgical interference is essential, has now become universal. With a few exceptions, when the use of chloroform is contra-indicated, for obvious reasons, it is the prelude to all operations of any magnitude. The advantages of anesthesia thus produced, in connexion with the major operations, are so great as far to outweigh the chances of any immediate or remote risk that may attend upon the chloroform inhalation. In a word, chloroformization is a permanent institution in surgical practice, not to be overthrown till a more perfect anesthetic has been discovered. Not so in obstetricity. Here opinion is much divided. After years of experience, it is admitted, by not a few, that the perfect immunity from danger, which was at first asserted to be peculiarly attached to it in labour cases, cannot now be accorded to it. There is no doubt that considerable danger is connected with the exhibition of chloroform, which, though not frequently experienced, is yet sufficient to contra-indicate its general use in ordinary labour, where, apart from its administration, no danger can be said to exist. By degrees this opinion is becoming more general in this country, and the indiscriminate administration of chloroform vapour in obstetric practice is by no means so frequently adopted by our practitioners, now, as it was soon after the discovery of the use of the agent. Many practitioners are of opinion, however, that there are some cases of labour in which chloroform inhalation is most beneficial, and such make a selection of cases in which to administer it. There are some who consider that the drug should be banished altogether from the practice of midwifery. In fact there exist three opinions on the subject, namely:—Firstly, that it should be given in all labour cases; secondly, that it should be administered only in certain selected cases; and, thirdly, that it should never be given at all. Prejudice

may, of course, influence each section of opinion; to me the middle course appears to be the one most consonant with reason. To assert that because the indiscriminate administration of chloroform in obstetric medicine, has sometimes proved dangerous, or even fatal, it should be therefore excluded from obstetric practice altogether, is simply to argue against its use from its abuse.

During my years of office, and those of my friend and colleague, Dr. George Johnston, in the Dublin Lying-in Hospital, we very frequently administered chloroform vapour, yet we never met with an immediately fatal casualty; nor a death *remotely* caused by its use, so far as we could form a judgment. It certainly was my opinion that there existed a disposition to draining, after and during the third stage of labour, in those cases in which the anesthesia had been complete, or who had been kept under the influence of the vapour for any considerable time. Nevertheless, there did not occur one single case of alarming hemorrhage which we could indisputably lay to its charge. Under no circumstances could I come to the conclusion that our death-rate was increased, disease induced, or convalescence prolonged, by the use of this agent.

Amongst the strongest opponents of chloroform in midwifery practice, in this city, was a gentleman who lately published a paper in a recent number of this Journal[a] against its use in that branch of medicine. It is with much regret that I have to state this gentleman is now no more. In the midst of a successful career, with a bright prospect before him, he has been unexpectedly hurried from this world by death, who has recently been so relentless towards the members of our profession in this country. In the paper alluded to the author has brought forward the statistics of Dr. George Johnston and myself to prove the baneful effects of chloroform inhalation in labour cases. In this paper our statistics are also contrasted in rather an unfavourable light with those of Dr. Collins and Drs. Hardy and M'Clintock. I should not, however, now, under the unhappy circumstances of the case, have obtruded these observations on the profession were it not at the express wish of the Editor of this Journal, at the desire of my friend and colleague, Dr. George Johnston, and last not least, for the interests of science. The paper in question, if carelessly perused, is one well calculated to mislead; and I should much regret if the sufferings of humanity were in the slightest degree left unmitigated, in consequence of the opinions and reasonings therein advanced. The allegations against

[a] Vol. xxxv., p. 353.

the administration of chloroform in midwifery practice in this paper are:—"That when inhaled during labour" chloroform "very fruitfully predisposes to hemorrhage, puerperal inflammation, chest affections, and to other diseases detrimental to health and life, which it aggravates if given during their presence." "It also lays the foundation of diseases to arise at a more distant period, and thus increases the mortality in child-bed, and subsequent thereto." And, lastly, that by its means puerperal fever, having been raised, "may become epidemicized." In support of these allegations reference is made to the published reports of the Dublin Lying-in Hospital, viz., to those of Dr. Collins, Drs. Hardy and M'Clintock, and Drs. Johnston and Sinclair. "Thus," continues the author, "in the first report are recorded 16,414 deliveries and 164 deaths, or 1 in 100; in the second, 6,634 deliveries and 65 deaths, or 1 in 102; whereas in the third, 13,748 deliveries are given, and 163 deaths, or 1 in 84!! *But of these last* 13,406 *cases were not chloroformed, of which only* 133 *died, or* 1 *in* 100; *and of the remaining* 342, *who took the drug*, 30 *died, or* 1 *in* 11!!!" So that the argument is—inasmuch as the mortality was less in Dr. Collins' mastership, and during the period in which Drs. Hardy and M'Clintock were assistants to the hospital, when chloroform was not administered, than it was when Drs. Johnston and Sinclair were assistants, when it was administered—therefore the increased mortality was caused by chloroform. Now, before I proceed to inquire into the question as to whether diseases, &c., were caused by chloroform during our term of office (*i. e.*, Dr. Shekleton's mastership), I shall lay before the reader what the mortality, as stated in each of these reports, really was; and, first, I shall quote from Dr. Collins. At page 365 is to be found the following table, showing the cause of death amongst those delivered during his term of office:—

"*Diarrhea,	1
*Typhus,	5
Rupture of uterus and vagina,	32
Uterine hemorrhage,	11
*Puerperal fever,	59
*Inflammation of brain,	3
*Ulceration of intestines,	3
Hectic fever,	1
*Grief, apparently,	2
*Stricture of intestines,	1
Effects of tedious and difficult labour,	11

Convulsions,	2
Sloughing of vagina,	6
*Hydrothorax,	2
*Pericarditis,	1
Peritoneal inflammation, placenta retained,	4
*Abscess in spinal canal,	1
*Lumbar abscess,	1
*Phthisis,	2
*Diffuse cellular inflammation,	1
*Abscess in abdomen,	2
*Acute bronchitis,	1
*Anomalous disease,	12

"Thus," continues Dr. Collins, "of the 16,414, 164 died, or in the proportion of 1 in 100. If from this number we deduct the deaths from puerperal fever, which may be considered accidental, the proportion becomes greatly diminished, viz., to 1 in 156 deliveries; and again, if we deduct those deaths from causes not the result of child-birth (which are marked * in the table) the mortality from effects arising in consequence of parturition is vastly reduced, viz., to 1 in 244."

I shall now turn to the report of Drs. Hardy and M'Clintock, which shows the mortality of Dr. C. Johnston's mastership for a period of three years, and comprises an account of 6,634 deliveries, of which 65 died, or 1 in 102. At page 2 of this report their mortality can be inspected as follows, an asterisk being "prefixed to those cases in which a non-puerperal disease was the cause of death:—

"Phlebitis and arthritis,	24
Ruptured uterus,	9
Peritonitis,	7
Sloughing of uterus and vagina,	6
Effects of hemorrhage	5
*Bronchitis and pneumonia,	3
*Phthisis,	2
*Anasarca,	1
*Scarlatina,	1
Convulsions,	2
Mania,	1
*Jaundice,	1
*Tumour on bronchus,	1
*Anomalous disease,	2

"Now," continues Drs. Hardy and M'Clintock, "if, as is but fair, we deduct the 11 cases, in each of which the cause of death was a strictly non-puerperal disease, and also four others, where the patients were, upon admission, in a dying state, the average mortality will be reduced to 1 in 132; and, further, if puerperal fever be regarded as an accidental occurrence, and therefore be excluded, the proportion of deaths will be vastly lessened, viz., to 1 in about 250."

If page 14 of Dr Johnston's and Sinclair's report (which gives the account of 13,748 deliveries, of which 163 died, or 1 in 84) be now consulted the following remarks and observations will be found:—"The proportion of women dying from all causes was 1 in 84, and nearly one-third. In order, however, to arrive at a correct idea of the mortality of the hospital, we may be permitted to deduct, from the total number of deaths, those who were admitted in a dying state, which latter amounted to 17. We thus leave a balance of 146 dying from all causes, or, *quam proximè*, 1 in 94. Then if we deduct, from the above 146 fatal cases, those whose deaths had arisen from visitations of puerperal epidemic, viz., 70 victims to that disease, we reduce the sum to 76 dying from all causes, or 1 in every 180⅔. But, upon examination of the 76 remaining fatal cases, we find that—

"1 died of abscess of the brain.
2 „ apoplexy.
3 „ kidney disease.
4 „ heart disease.
3 „ bronchitis.
1 „ pleuritis.
3 „ pneumonia.
7 „ phthisis.
2 „ dysentery.
7 „ typhus.
2 „ scarlatina.
1 „ erysipelas.
1 „ rupture of vessel in mesentery.
1 „ perforating ulcer of stomach.
2 „ mental anxiety.

"In all 40 deaths from other than puerperal causes. If, then, we deduct these from the 76 fatal cases, there remain 36 deaths which originated in labour, or 1 in every 381½." This, then, is our absolute mortality, reckoning those, even, who were admitted in a dying state. Moreover, there are included, amongst these deaths

which I have here set down as originating in labour, "three cases concerning which considerable doubt existed as to whether death did really result therefrom," in other words, such as the other authors describe as "anomalous." Now, comparing these accounts in the superficial manner in which the author of the paper against chloroform has compared them, Dr. Shekleton's mortality does certainly appear to a considerable disadvantage beside that of Dr. Collins', and the mortality during three years of Dr. C. Johnson's mastership, standing as they do thus, respectively, 1 in 84; 1 in 100; 1 in 102. But, making the proper deductions to arrive at the *absolute* child-bed mortality, we find the following results on comparison.

Deducting Dr. Collins' 12 anomalous and his 24 non-puerperal deaths, and his mortality is lessened to 1 in 128.

Deducting Dr. Hardy's and M'Clintock's 4 cases dying on admission and their 11 non-puerperal deaths, and their mortality is reduced to 1 in 132.

Deducting Dr. Johnston's and Sinclair's 17 cases dying on admission and their 40 non-puerperal deaths, and their mortality is reduced to 1 in 129.

Still further reducing Dr. Collins' deaths by those who fell victims to puerperal fever, and his mortality becomes so low as 1 in 237.

Submitting Dr. Hardy's and M'Clintock's fatal cases to a similar deduction, and their mortality is reduced to 1 in 250.

Applying the same process to Dr. Johnston's and Sinclair's deaths, and their mortality is reduced to 1 in 381.

So that the absolute child-bed mortality stands respectively for Dr. Collins, Drs. Hardy and M'Clintock, and Drs. Johnston and Sinclair, thus—1 in 237, 1 in 250, 1 in 381.

That being the state of the absolute mortality from the effects of labour during the periods over which the respective reports spread; were I to argue after the manner of the late Dr. Johns, I might come to the conclusion: that, since chloroform was extensively administered, during Dr. Shekleton's mastership, to the women then delivered in the Dublin Lying-in Hospital, and the mortality from the absolute effects of labour was so low as 1 in 381; and as this anesthetic was not given during the masterships of Drs. Collins and C. Johnson, when the absolute childbed mortality was 1 in 237 and 1 in 250 respectively; therefore, it was to the *frequent* administration of chloroform that this immunity from death originating from labour was due.

The great cause of the increase in the general mortality of the

hospital during Dr. Shekleton's mastership was evidently puerperal fever—the cases attacked by that disease bearing the ratio to those delivered of 1 to every 106; while during Dr. Collins' mastership the ratio was 1 to 189 of those delivered. I regret I cannot make a computation of the three years of Dr. C. Johnson's mastership, Drs. Hardy and M'Clintock not giving tables of that epidemic; I presume, however, that it was less than during Dr. Shekleton's mastership; certainly not greater. It therefore remains to see whether chloroform was the cause of this frequency of attacks of puerperal fever; or whether, *induced by its means*, it became *epidemicized* during Dr. Shekleton's mastership. I therefore submit the following table, drawn out, at my request, by my friend and colleague, Dr. George Johnston, who has kindly compiled for me several other tables, from which I shall presently deduce other facts tending to elucidate this subject. This table exhibits the number of deliveries and deaths; the number of cases which were put under the influence of chloroform vapour during labour, and those who, after such treatment, died; the number of peritonitis cases, and deaths from that disease; and, lastly, the cause of death in those cases who happened to die after having been delivered under the influence of chloroform, for each year over which our report runs:—

Year	Number of Deliveries	Deaths	Chloroform given		Peritonitis during		Cause of Death in those Cases who had Chloroform.										
			Cases	Died	Cases	Deaths	Pneumonia	Convulsions	Ulceration of Intestines	Scarlatina	Mania	Peritonitis	Rupture of Uterus	Rupture of Mesenteric Veins	Phlebitis	Iliac Abscess	Diarrhœa	Sloughing
Nov. & Dec. 1847	242	17	0	0	10	9	..	..	..	..	..	..	..	..	..	..	..	..
1848	1,823	33	38	1	43	23	1	..	..	..	..	..	..	..	..	..	..	..
1849	2,063	37	39	6	29	19	..	1	1	1	..	2	1	..	..	..	..	..
1850	1,982	17	47	4	15	2	..	2	..	..	..	1	..	1	..	..	..	..
1851	2,070	14	36	3	10	5	..	..	..	..	..	2	..	..	1	..	..	..
1852	1,963	11	53	6	3	2	..	2	..	..	..	2	1	..	..	1	..	..
1853	1,926	17	59	5	8	5	..	1	..	..	..	2	1	..	..	..	1	..
1854	1,679	19	41	5	11	10	..	..	..	..	1	3	..	..	..	..	..	1
Totals,	13,748	165	313	30	129	75	1	6	1	1	1	12	3	1	1	1	1	1

Now, it will be seen, by referring to the above table, that puerperal peritonitis was pretty well epidemicized *before chloroform was introduced into the hospital.* The epidemic was in that institution when Dr. Shekleton received it from his predecessor. This is evident, for, during the months of November and December, 1847, there were 242 deliveries and 17 deaths, or 1 in 17, and 10 cases of puerperal peritonitis, in other words, 1 out of every 24 of those then delivered were attacked; yet not a particle of chloroform was inhaled during that period.

	Mortality from all causes.		Peritonitis.		Chloroform.
1848	1 in 55	...	1 in 42	...	once in 48
1849	1 in 53	...	1 in 71	...	once in 52
1850	1 in 116	...	1 in 132	...	once in 42
1851	1 in 147	...	1 in 207	...	once in 57
1852	1 in 178	...	1 in 654	...	once in 37
1853	1 in 113	...	1 in 240	...	once in 32
1854	1 in 88	...	1 in 152	...	once in 40

The years in which chloroform was most frequently exhibited were 1850, '52, '53, and '54; and it will be seen that during this period of years 7,550 women were delivered, of whom 64 died, making a mortality of nearly 1 in 118. During this period chloroform was administered once in every 37 deliveries. While the years in which chloroform was less frequently administered were the two last months of 1847, '48, '49, and '51, during which period there were 6,198 women delivered, of whom 101 died—a mortality of 1 in 61—while chloroform was administered but once in nearly every 55 deliveries. Therefore, arguing after the manner of the late Dr. Johns, I might, from the above facts, come to the conclusion, that since the mortality, during the years in which chloroform was most frequently administered, was less than it was during the years in which it was not so frequently given, the lessened mortality was due to the more frequent use of chloroform.

A glance at that portion of the table which exhibits the causes of death of those who had taken chloroform, in conjunction with the chloroform column to the left, shows that of the 38 cases who took chloroform in 1848 but 1 died, and that of pneumonia. In 1849, 39 women took chloroform, of whom 6 died; 1 was a case of puerperal convulsions, 1 died of ulceration of the intestines, 1 of scarlatina, 2 of peritonitis, and 1 of rupture of the uterus. In

1850, 47 took chloroform, and of these 4 died; 2 were cases of puerperal convulsions, 1 of peritonitis, and 1 of rupture of a mesenteric vessel. In 1851, 36 took chloroform, of whom 3 died—2 of peritonitis and 1 of phlebitis. In 1852, 53 women inhaled chloroform, and 6 of these died; 2 were cases of puerperal convulsions, 2 died of peritonitis, 1 of rupture of the uterus, and 1 of iliac abscess. In 1853, 59 women took chloroform, of whom 5 died; 1 was a convulsion case, 2 died of peritonitis, 1 of ruptured uterus, and 1 of diarrhea. In 1854, 41 took chloroform, of whom 5 died—1 of mania, 3 of peritonitis, and 1 of sloughing.

It is most curious, the mode in which it is attempted to exhibit the baneful effects of chloroform as regards mortality. "But of these last," says the late Dr. Johns, alluding to our 13,748 deliveries, "13,406 *cases were not chloroformed, of which only* 133 *died, or* 1 *in* 100! *and of the remaining* 342 *who took the drug*, 30 *died, or* 1 *in* 11!!!" This statement appears at first sight truly appalling; but when we come to inquire as to the nature of these 342 cases who took chloroform, and find that with the exception of 18 or 20 (to whom the drug was given during perfectly natural labour, and concerning whom we shall presently speak), they come under the head of one or other of the following category, we shall not be quite so surprised at the large death-rate allotted to them. Thus, as many as 144 of the 342 cases were of the order tedious labour, and were subjects of the forceps. Laborious labour, terminating in the perforator and crotchet, claimed 86; convulsions, 12; and the remainder were comprised under tedious labour, terminating after unnatural delay, unaided by instrumental interference, together with every variety of preternatural labour, of course including transverse presentations, terminating in version, and the same completed by evisceration. With the exception of a few cases of natural labour in which the drug was administered to try its effects, chloroform was only given under the circumstances enumerated above. Is it fair, then, to take unnatural conditions which of themselves may be said to be almost the only causes of absolute child-bed mortality; and because chloroform was administered under such unnatural conditions, to argue that the death-rate in such a series was due to that drug.

I shall now submit in a tabulated form the histories of those cases who during their labours had inhaled chloroform, and subsequently died. This table exhibits the number of pregnancy, the number of hours in labour, the mode of delivery, and the cause of death:—

MORTALITY WHERE CHLOROFORM WAS USED DURING LABOUR

No. of Pregnancy		Hours in Labour	Sex of Child		Result to Mother Date of Death	Mode of Delivery				Convulsions	Peritonitis	Phlebitis	Sloughing	Iliac Abscess	Placenta Previa	Accidal. Homge.	Rupture, Uterus	Rupture of Mesenteric Vein.	Scarlatina	Pneumonia	Ulceration of Intestines	Anxiety
1st	Subst		M	F		Forceps	Craniotomy	Turning	Natural													
1	—	40	*l*	—	12th day	F	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—
1	—	8	—	D	3rd „	—	C	—	—	C	—	—	—	—	—	—	—	—	—	—	—	—
1	—	10	—	D	14th „	—	—	T	—	—	—	—	—	—	—	—	—	—	—	—	1	—
–	11	11	—	Dp	7th „	—	—	—	N	—	—	—	—	—	Pla P	—	—	—	—	—	—	—
1	—	32	—	D	6th „	—	C	—	—	—	Ps	—	—	—	—	—	—	—	—	—	—	—
1	—	40	D	—	3rd „	—	C	—	—	—	Ps	—	—	—	—	—	—	—	—	—	—	—
–	3	96	—	Dp	2nd „	—	C	—	—	—	—	—	—	—	—	—	R	—	—	—	—	—
1	—	90	—	D	3rd „	F	—	—	—	—	—	—	—	—	—	—	—	Mev	—	—	—	—
1	—	21	*l*	—	3rd „	—	—	—	N	C	—	—	—	—	—	—	—	—	—	—	—	—
–	2	16	D	—	2nd „	—	C	—	—	—	Ps	—	—	—	—	—	—	—	—	—	—	—
1	—	13	—	D	5 hours	F	—	—	—	C	—	—	—	—	—	—	—	—	—	—	—	—
1	—	38	D	—	3rd day	—	C	—	—	—	Ps	—	—	—	—	—	—	—	—	—	—	—
–	6	30	D	—	9th „	—	C	—	—	—	Ps	—	—	—	—	—	—	—	—	—	—	—
1	—	42	—	D	7th „	F	—	—	—	—	—	Phs	—	—	—	—	—	—	—	—	—	—
1	—	42	D	—	4th „	—	C	—	—	—	Ps	—	—	—	—	—	—	—	—	—	—	—
–	3	24	—	D	10th „	— 2	C	—	—	—	—	—	—	Ilc Abs	—	—	—	—	—	—	—	—
–	3	9	—	*U*	24 hours	F	—	—	—	C	—	—	—	—	—	—	—	—	—	—	—	—
1	—	7	—	*l*	2nd day	—	—	—	N	C	—	—	—	—	—	—	—	—	—	—	—	—
–	4	60	D	—	4th „	—	C	—	—	—	Ps	—	—	—	—	—	—	—	—	—	—	—
–	9	36	D	—	1 hour	—	—	T	—	—	—	—	—	—	—	Acl Hge	—	—	—	—	—	—
1	—	20	*l*	—	3rd day	F	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Anx
–	6	45	—	D	Same day.	F	—	—	—	—	—	—	—	—	—	—	R	—	—	—	—	—
1	—	9	—	Dp	2nd „	—	—	—	N	C	—	—	—	—	—	—	—	—	—	—	—	—
1	—	47	—	D	5th „	—	C	—	—	—	Ps	—	—	—	—	—	—	—	—	—	—	—
1	—	33	D	—	17th „	—	C	—	—	—	—	—	Sg	—	—	—	—	—	—	—	—	—
–	2	60	D	—	6th „	—	—	—	N	—	Ps	—	—	—	—	—	—	—	—	—	—	—
1	—	53	*l*	—	2nd „	F	—	—	—	—	Ps	—	—	—	—	—	—	—	—	—	—	—
1	—	27	D	—	4th „	—	C	—	—	—	Ps	—	—	—	—	—	—	—	—	—	—	—
1	—	26	—	*l*	5th „	F	—	—	—	—	—	—	—	—	—	—	—	—	Sca	—	—	—
–	9	28	—	D	12th „	—	C	—	—	—	—	—	Sg	—	—	—	—	—	—	—	—	—
19	11	1,014	14	17	—	9	14	2	5	6	11	1	2	1	1	1	2	1	1	1	1	1

From the above table it will be seen, that of the 30 fatal deaths of the 342 chloroform cases, 9 were subjects of the forceps; 14 of the perforator and crotchet; and but 5 were delivered unaided,

in one of whom the labour lasted for sixty hours, and who died of phlebitis; in another, the labour was complicated with placenta previa, and in the remaining three with convulsions. It will also be seen that the average duration of labour in these cases was 33·8 hours; the majority of them first labours (19), and 9 of these primipara ranged from 30 to 41 years of age.

With respect to the histories of the 9 fatal forceps cases, in addition to the information conveyed in the above tables, I find, that the first was admitted in acute bronchitis, which merged into pneumonia, the latter proving fatal. This case was, without doubt, one which ought not to have been subjected to chloroform inhalation. At present no one would attempt to administer chloroform, during labour, while the patient laboured under acute bronchitis; but this case occurred in 1848, when we were inexperienced in the use of the drug.

The second fatal forceps case could not, it may be granted, have been originated by chloroform.

The third was complicated, with convulsions.

The fourth, suffered from delay in the second stage of labour, from disproportion, and was seized, on the third day, with phlebitis.

The fifth was a twin labour, complicated with convulsions.

The sixth, suffered from delay chiefly in the second stage of labour, from disproportion. From the first approach of labour this woman had been in a state of great despondency and anxiety. She frequently exclaimed, during the progress of her labour, that she "was convinced her death was inevitable." On *post mortem* examination nothing could be found sufficient to account for her death, nor could any clue be obtained whereby to arrive at the source of her mental disquietude. Chloroform was given only just prior to operation.

The seventh was permitted to continue too long in the second stage, and rupture was the consequence.

The eighth took peritonitis after a prolonged second stage.

The ninth contracted scarlatina.

Remove now the case of death from pneumonia, and is there anything peculiar in the remaining eight cases, which point to chloroform as a cause of death? Exclusive of labour cases complicated with convulsions, we are possessed of the recorded facts of 144, in which the forceps was used. The operation was resorted to in these either on account of inertia of the uterus, some disproportion which could be overcome by means of the instrument, or malposition of

the fetal head. Every one of these cases was put under the full influence of chloroform vapour prior to operation, and a complete anesthetic condition was maintained during the process of extraction, which occasionally occupied a considerable time, yet we can find but 6 fatal cases out of these 144. Surely, to say the least, this rate of mortality is not more disheartening than that of forceps cases prior to the introduction of the chloroform vapour. I may add, the average duration in hospital of these 144 forceps cases was about 9 days.

With respect to pure crotchet cases (*i.e.* irrespective of complex labours, such as convulsions, placenta previa, accidental hemorrhage, rupture of the uterus; and also exclusive of those connected with preternatural presentations) we have the records of 85, the average duration of labour in which, amounted to something over 34 hours. The cause of interference in these cases was—disproportion not suited to the forceps; inertia when the child was dead; prolapse of funis, in which the child was dead; deformity of the pelvis, either general or partial, and sometimes to an extreme degree; and, lastly, more or less occlusion of the vagina from cicatrices, the result of former inflammations and sloughings. Of these 85 cases but 14 died, viz.: 8 of peritonitis, three of sloughing, and 1 of iliac abscess. Now, all these cases were subject to the full influence of chloroform—many of them for a considerable period; yet I consider it difficult to arrive at the conclusion that chloroform originated a mortality of 1 in 6. The causes of deaths are perfectly natural under the circumstances, viz., peritoneal and pelvic inflammation. Our mortality in crotchet cases would have been much less was it not that the forceps were frequently, and, for the most part, early used in tedious labours. Many cases that would have incurred no great risk had they been submitted to the perforator or crotchet, as they would have been some years previous, were thus removed from the sphere of these instruments; and, consequently, all our crotchet cases were, with a few exceptions (*i.e.*, in which it was resorted to to cut short labour in a few cases, when the child was known to be dead) of the most formidable kind. With respect to puerperal convulsions, always a complication of a most serious nature, one having an average mortality of 1 in $4\frac{1}{2}$, according to Churchill as collected from twenty authorities, we have recorded 50 cases and 13 deaths, or nearly 1 in 4; while chloroform was given to but 12, of whom 6 died. Those who inhaled chloroform were all seized with convulsions *before* it was administered; they were the worse description of cases, and I

cannot bring forward any evidence to prove that it was the cause of death in any, or that it accelerated death. I may state, that in our report, Dr. Johnston and I express ourselves as follows:—"As to the exhibition of chloroform" (*i.e.*, in convulsions) "nothing satisfactory could be said; it was, however, seldom given, and never unless depletion had been previously practised."

If it is desired to discover the true effect of chloroform in labour cases in the production of disease and death, the candid inquirer would seek for cases of the most uncomplicated description. In other words, he would endeavour to find the results of chloroform inhalation in labours purely natural, or nearly so. I submit, therefore, the following table of such cases. It contains, however, two cases of breech presentation:—

Chloroform in Natural Lobour.

No.	Age	No. of Pregnancy		Sex of Child	
		First	Subsequent	Male	Female
1	27	–	2	1	–
2	35	–	9	1	–
3	26	–	4	–	1
4	24	1	–	–	1
5	27	–	3	1	–
6	30	–	4	1	–
7	18	1	–	1	–
8	22	1	–	–	1
9	22	1	–	1	–
10	28	1	–	–	1
11	30	–	2	–	1
12	19	1	–	–	1
13	36	–	8	–	1
14	24	1	–	1	–
15	40	–	11	–	1
16	22	–	2	–	D
17	21	1	–	1	–
–	–	8	9	8	9

In these cases there was no need of operative interference. Chloroform was given to try its effect, and they were all delivered under its influence, and maintained in a state of anesthesia more or less complete, for periods varying from half an hour to four hours and a-half. Yet here we have not a single fatality; and, with two exceptions, not one remained longer in hospital than the ninth day. Convalescence was not prolonged, save in the two cases alluded to, No. 10, who became affected with emphysema from over-straining, and No. 13, who became maniacal.

No. 13 became violently maniacal on the sixth day after delivery; all means failed in attempting to combat this state, till chloroform was *again* had recourse to, which had a marvellous effect in tranquilizing her; was persevered in as necessity required, and was finally the means of restoring her.

In cases Nos. 2, 9, and 16, there was a tendency to draining, but no decided hemorrhage.

It is not my intention to enter into a prolonged dissertation on these facts. My object is not to offer a full reply to the late Dr. John's paper, but merely to lay before the profession, the statistics of Dr. George Johnston and myself, with respect to their connexion with chloroform, in order that I may put the following questions:—

From a careful review of these statistics, can any one arrive at the same conclusions as the late author, viz., that chloroform, when inhaled during labour, "*very fruitfully* predisposes to hemorrhage, to puerperal inflammation, chest affection, and other diseases detrimental to health?" That it "lays the foundation of disease, to arise at a more distant period, and thus increases the mortality in child-bed and subsequent thereto?" That, in fine, puerperal fever becomes, through the means of chloroform, "epidemicised?" It is in support of these "positions" he referred "to the published Reports of the Dublin Lying-in Hospital;" and, it is from the comparison of our report with those of Drs. Collins, Hardy, and M'Clintock, that he arrived at his conclusions?

In the last series of cases I have given, viz., those to whom chloroform was given in natural labour there certainly appears a case of puerperal mania; but strange to say, chloroform was the agent which subsequently subdued the disease after all others had failed. I may here, also, observe, that it is stated in the paper alluded to, that I have recorded cases "in which puerperal convulsions occurred after its employment." Yes, doubtless; but I have recorded no case in

which convulsions did not occur in those cases *previous* to its employment. It was given with the hope of checking such seizures. It is said "Sinclair records one case of epilepsy," implying that such a condition was induced by chloroform. There is no case of epilepsy mentioned in our report, save that of an old epileptic who was seized with epileptic fits after her confinement, and who never, during her stay in hospital, inhaled a particle of chloroform.

Nevertheless, I am not an advocate for the indiscriminate administration of chloroform. There is no doubt that a certain risk attends its administration—a risk I do not consider we have a right to run in cases of labour *purely natural*. I was always under the impression that a tendency to hemorrhage was sometimes induced by its use, and, of course, a degree of uterine inertia. The following case is a good illustration of these effects of chloroform which I take the liberty of laying before my readers:—

Case I.—Towards the latter end of 1856, I was sent for to see A. B., who required my attendance at her third accouchement, expected to take place in the ensuing January. She was a healthy young woman, a brunette, and of sanguine temperament. I was informed that, at her two previous confinements she had taken chloroform, that it agreed with her amazingly, and that her attendant (a gentleman of great eminence) had told "her never to be confined without it," and "to take PLENTY of it without fear." I was likewise informed, that she had always severe "floodings" after the birth of the child; but that this was owing to "adhesion of the after-birth" requiring the introduction of the hand for its removal. I could not ascertain the period over which her labours ran. She did not complain of fixed pain or uneasiness over any portion of the uterine tumour. Her general health was most excellent. Four years had transpired since the birth of her last child, which, together with her first-born, was a healthy boy.

Towards the latter end of January I was called to A. B. in labour. I arrived very soon after its commencement, having been sent for immediately on the first accession of pain; my patient being most anxious to be lulled into forgetfulness by chloroform, as soon as possible. The first stage must have been very rapid, for, on my arrival, I found the head well down in the pelvis, and uterine action very energetic; in fact three or four good pains would have terminated the labour. I asked her, could she summon up sufficient courage to bear one or two more pains; and I promised, that if she

was not then delivered I should give her chloroform. This she would not listen to, and requested me, rather angrily, to administer it "at once," and to "give" her "plenty of it." I accordingly put a drachm in the hollow cone formed by a cambric handkerchief and gradually brought it to her mouth, just at the commencement of the next pain. She had inhaled it only a few seconds when the pain, instead of accumulating in force, completed itself with feeble action. I kept her under the partial influence of the drug, giving it to her at the commencement of each pain, for several pains; but not one was sufficiently strong to act on the head, and all voluntary efforts ceased. I was in hopes that by persevering the natural action of the uterus would return, as I had often observed to be the case; but I was disappointed. I then allowed her to get from under the influence of the drug, and tried to persuade her to bear a "pain" without chloroform; but she would not hear of it. I therefore put her into a state of complete anesthesia. The pains still continued at regular intervals, but, as before, were perfectly useless. In this state matters continued—I varying the depth of anesthesia for nearly six hours. At the termination of this time, since she was as far as ever from delivery, I determined to use the forceps; and called to my assistance my friend, Dr. Dwyer, for whose opinion, as an obstetric physician, I entertain the highest respect. Dr. Dwyer satisfied himself as to the paralyzing effect of the chloroform; and after watching the case some time with me, considered, that as matters seemed likely to remain in *statu quo* for an indefinite period, it would be well to cut short the labour at once. I consequently delivered her of a healthy boy, with the forceps. The uterus was sluggish, and in a few minutes, though constantly commanded by the hand, very brisk hemorrhage set in. The placenta was pressed off; there were no adhesions. To arrest the hemorrhage we were obliged to administer ergot, to use cold applications, and to give different stimulants. At last we procured a permanent contraction, and secured the uterus with pads and binder. Our constant attention was required in the room for a considerable period after delivery. This lady made a most excellent recovery, never having exhibited a bad symptom during her convalescence, which was but of ordinary duration.

Upon the consideration of this case no one, I think, can arrive at any other conclusion than that the effects of the vapour of chloroform was to paralyze the uterus. For my part I am perfectly positive that had this lady endured three or four more pains without its use,

she would have been delivered in less than half-an-hour after my arrival. That being granted, the hemorrhage was consequent upon want of action also. So that, here was a case of purely natural labour, whose duration would have, most certainly, been but about an hour and a-half, by means of chloroform inhalation prolonged to a period of several, and the duration of which promised to have been indefinite—a case of labour changed from natural to instrumental; and rendered also one of danger from the occurrence of hemorrhage during the placental stage. I could not, as I before mentioned, find out how long this patient had been in her previous labours, or whether she was delivered naturally or otherwise; but this I have no doubt of, viz., that "the plenty of chloroform" which was on those occasions administered to her, and "agreed with her so charmingly," was the cause of her floodings, by giving rise to inertia. Moreover I consider that the probability was she never had an adherent placenta; though she might have had a retained one requiring removal. For the sake of obliterating, in this case, a few minutes' pain, a natural process, which almost invariably terminates happily, was rendered one of considerable risk.

The following is so instructive a case that I need no apology for detailing it:—In August, 1859, I was required to attend C. D., in her first confinement; she resided in the suburbs. This lady was most anxious to have chloroform, having been urged to it by some of her female acquaintances, so that I had to promise that it should be in the room during her labour; and I further stated that should I consider it necessary I would administer it. This was a premature labour—feet presented—and, as far as I could learn, of about the seventh month. She was taken ill at 5 o'clock, a.m., and when I arrived, at 7, a.m., the os uteri was sufficiently open to determine the presentation. She was a fine healthy young woman, aged twenty-five, and of sanguine temperament. I managed to coax her to forego chloroform for some time—till the feet were external and breech in the os uteri; but her fortitude now forsook her. She prayed so hard for the vapour, and I was so urged by her sister (a woman who had had many children), that I proceeded to administer it at the next pain. As in the former case, into the hollow cone, formed by a cambric handkerchief, I poured about a drachm of chloroform, and brought it very gradually towards her face. I had the fingers of my disengaged hand on the pulse. Uterine action was arrested at once, and she had scarcely inhaled it half a minute when I felt the pulse suddenly failing.

Immediately removing the handkerchief I was horrified to find her lips pale, and that respiration had, to all appearance, ceased. I at once fanned off the chloroformic atmosphere from her face, drew out her tongue, had her aspersed freely with water, and employed all the rough-and-ready measures at hand in order to restore respiration. To my inexpressible relief respiration returned, the pulse revived, and in a few minutes more she was out of danger. After some further delay, and upon the recurrence of pain, I completed the delivery. The child died a few minutes after birth. It was a monstrosity, deficient in the mesial line both anteriorly and posteriorly. The placenta was naturally expelled, and convalescence uninterrupted.

I do not think any one can doubt but that in this case life was very nearly lost; probably had my finger not been on the pulse the woman would be now in her grave. The danger here could not be accounted for by fault on the side of the administrator, who was thoroughly versed in its exhibition; neither could it be laid at the door of the impurity of the drug, for it was fresh, and from one of the most eminent houses in the city; nor was it because of the health of the patient, who was perfectly sound in every respect. The danger originated from some cause beyond our ken; it was wholly unexpected. Some idiosyncrasy against the drug must exist with some; and we know not when such a subject may come under our treatment. It was needless to give this patient chloroform, and a trivial case was nigh made a fatal one.

The observations of Dr. Meigs, of Philadelphia, relative to the administration of anesthesia in labour, are most sound, and, though enunciated some ten or twelve years ago, during, it may be said, the infancy of anesthesia, have, in my opinion, been strengthened by experience. He observes:—[a] "1st. As to the necessity in ordinary cases of parturition. The average duration of labour is four hours; and I have shown (page 252) that the number of labour pains is about fifty, and that they last, each, about thirty seconds; so that the parturient woman really suffers from labour pains about fifteen or sixteen minutes, and no more; and these sixteen minutes are distributed among the four hours of labour of mean duration.

"It has never been pretended that the motive for the anesthetic practice has any connexion with the other pains of woman in labour, but only with the suffering from *contraction* or *labour pains;* for,

[a] Obstetrics, the Science and the Art. Philadelphia, 1849. P. 316.

though we may well suppose that women suffer from painful sensations independent of those arising from the actually contracting womb, yet we find them, in general, *easy*, *complacent*, and but too happy when the pain is off. Hence the ether is exhibited for the pain, and for no other motive.

"I contend that it is to an exaggerated notion of the nature of labour pains we owe the introduction of ether into our art; for if the mean of labour pain be only fifteen minutes in all, there can be no necessity in *the average of cases for its exhibition.* I should find the objection to it less, and the inducement greater, were the fifteen minutes of pain to be always fifteen consecutive minutes. When they are distributed through two hundred and forty minutes, or four hours, I look upon the exhibition as unnecessary and uncalled for.

"2nd. The representations that have been made by the friends of anesthesia, of the harrowing distress endured by women in childbirth, do not consist with the general state of facts in the case; and it is quite as true that the lying-in room is, for the most of the scene, a scene of cheerfulness and gaiety, instead of the shrieks and anguish of despair.

"Few women lose their health or their lives in labour, and the dread of future sufferings is insufficient to prevent the increase of the family, &c." Then, after referring to the effect of anesthetics upon the encephalon, and that there exists but a slight step from anesthesia to death, the influence, perhaps, extending to the medulla oblongata, he concludes:—"Now I have to suggest to the student the propriety of asking what would be his feelings provided in any such case this silver cord should be loosened; and I ask him whether, if the anesthesia should proceed at first or secondarily, to attack and overthrow the power of the medulla oblongata, his patient would not instantly be deprived of life." . . . "I do not well understand how those persons can recover their complacency who, by an unnecessary and inappropriate resort to so dangerous a process, *have seen the victims of this extraordinary power struck lifeless before their eyes!*"

Now although amongst the facts here recorded not one fatal accident from chloroform can be found; though it cannot be said that mortality was increased in any way by its means; or that disease, on account of its exhibition, was rendered more rife, or convalescence prolonged; though evidence sufficient cannot be obtained, from the perusal of these observations, to cause its utter condemnation and expulsion from obstetric practice; still, in my opinion, sufficient *can*

be gleaned to enable us to come to the conclusion, that the indiscriminate exhibition of chloroform vapour in labour cases should be abandoned, and that it should never be given in labour purely natural, or nearly so. It is true that out of all the cases Dr. Johnston and I have recorded, derived from our hospital experience, but on two or three occasions did symptoms sufficiently alarming occur to cause us to desist in its administration; and it is equally true that from out of my own private practice I can adduce but two cases strongly contra-indicating it; still these two, taken alone, are quite sufficient, in my opinion, to sever chloroform from its much too intimate connexion with natural labour.

In the first case I have detailed I would, without hesitation, have staked my professional reputation that in three or four "pains" after my arrival, the patient would have completed her delivery. That is, physical pain by no means unendurable (this was a third labour), and which, if continuous, would have occupied no more than two minutes of time. Yet, to save herself from even this (in consequence of previous treatment), a woman insists upon being rendered insensible, by chloroform, and thereby incurs the risk of the effects of hemorrhage, and renders what would have been a most easy labour, one without power, and necessitating the forceps. Even if the second case was the only one I had brought forward, it would in itself be sufficient to deter one from the indiscriminate administration of chloroform in labour. Here is a woman, in premature labour, at a period when a perfect command can be had over the labour, so that it can be terminated at once if necessary (the feet having been without the vulva and nates through the os uteri), who urgently begs for chloroform prior to its completion—a labour which might be considered all but natural, as far as delivery was concerned—chloroform is reluctantly given to her, and, but that the finger of the administrator is on the pulse, she is inevitably lost. I cannot help coinciding with the old argument, so far as chloroform inhalation is connected with natural labour, viz., that if 999 women took it during natural labour, with impunity, and but one was killed, that alone, should be sufficient to deter us from administering it in such cases. It is true no fatal case from chloroform in obstetric medicine has as yet been recorded; here, however, is detailed one very nearly so; and I can scarcely believe but that, if the histories of labour cases was searched, by candid and unbiassed investigators, we should find many other such examples. It may be asked, then, what are the cases in which

I recommend chloroform; or, in other words, in which I should not hesitate to administer it.

To enter upon this subject, at present, would extend an article already of too considerable length; so that, I shall reserve my further observations for a future number of this Journal.

My chief object in the present paper is to exhibit the facts published by Dr. George Johnston and myself, taken in connexion with chloroform inhalation during labour, in their true light; and to obviate such erroneous conclusion as might be arrived at, from a hasty perusal of the paper published in May, 1863, by the late Dr. Johns.

ART. VIII.—*Case of Fibro-Cystic Tumour of the Uterus.* By T. SPENCER WELLS, F.R.C.S., Surgeon to Her Majesty's Household, to the Samaritan Hospital, &c.

ON the 20th of June, 1864, I arrived in Dublin, having been requested by Dr. Stokes to come prepared to operate in a case which he and Dr. Beatty considered did not admit of delay. I saw the patient at once with Dr. Stokes. She was a single lady, forty-five years of age, extremely emaciated, but in excellent spirits. Dr. Stokes had detected two apparently solid tumours in the abdomen ten years before. One appeared to be central, and a little above the umbilicus. The other to the right side, under the anterior superior spinous process of the ilium. They were then, each, about the size of a goose egg. Increase had been slow at first; and no alteration in dress had been called for till a year ago. During the past two months increase had been very rapid. The abdomen was enormously distended, measuring fifty-six inches in girth at the level of the umbilicus, nineteen inches from the ensiform cartilage to the umbilicus, sixteen from umbilicus to symphisis pubis, twenty-three from the right anterior superior spine of the ilium to the umbilicus, and nineteen inches from the same process on the left side to the umbilicus. The greater prominence on the right side was very visible; the skin covering the umbilicus was distended by fluid simulating an umbilical hernia. Above the umbilicus fluctuation was very evident; but the fluid was evidently free in the peritoneal cavity, and covered a solid or semi-solid tumour that could be felt on displacing the fluid by deep pressure. Some of

the superficial abdominal veins were dilated, but were not varicose; the fluctuation below the umbilicus was very indistinct, and the tumour appeared to be adherent. Examination *per vaginam* showed the uterus was high, but central; the os virginal; the cervix absorbed or atrophied, and behind it a small portion of the tumour could be felt through the vaginal wall. The uterine sound passed to three and a-half inches. Menstruation had passed off quite naturally early in June; but there had been no appearance for the previous months. Up to that time she had been quite regular. There was no history either of excess or deficiency. The left leg was slightly œdematous, and she had occasionally felt it weak and painful. She had never been tapped.

The diagnosis which I made and wrote down was:—"A quantity of fluid free in the peritoneal cavity above the umbilicus—ascitic or ovarian? Below the umbilicus a large attached multilocular cyst." In consultation with Drs. Beatty, Gordon, and Stokes, it was agreed that I should tap above the umbilicus, and if the tumour appeared to be firmly adherent do no more; but if the tumour was not attached, to remove it. Accordingly, Mr. Macnamara having administered chloroform, and with the kind and able assistance of Drs. Beatty and Gordon, I tapped, with a very long trocar, above the umbilicus, and removed about thirty pints of clear rather viscid fluid. When all the fluid had escaped, the canula (which is fourteen inches long) was passed in all directions between the surface of the tumour and the abdominal wall, proving that there were no adhesions within reach. Fluctuation was also detected in different parts of the tumour. After removing the canula, and closing the small opening, I made an incision below the umbilicus about six inches long, and exposed what appeared to be two ovarian cysts separated by a deep fissure. I tapped that on the left side, and about ten pints of bloody serum escaped; two or three pints more of similar red fluid escaped after puncturing again within the cyst first opened, by pushing on the trocar without removing the canula. The tumour was then withdrawn, and found to have two attachments—one above to the tumour on the right side, and one below to the uterus. The former attachment was broken through, and two bleeding vessels on the torn surface of the right tumour were secured by silk ligatures. The left broad ligament was then transfixed, tied in two halves with strong silk, and the tumour was cut away. It then became a question what should be done with the tumour on the right side; and looking to its great size, solidity, evident close

connexion with the transverse colon, and with the omentum which contained some enormously distended veins, it was decided, with the full concurrence of Drs. Beatty and Gordon and Mr. Macnamara, that no attempt to remove this tumour should be made, especially as the patient was becoming very feeble. The wound was accordingly closed, and the patient placed in bed. She was extremely feeble, and brandy was administered freely; but she never rallied, nor recovered consciousness, continued to sink, and died about three hours after she had begun to take chloroform.

The following description of the tumour which I removed is the report of Dr. Ritchie, who examined it with great care twenty-four hours after removal:—

"The tumour is an irregularly flattened oval, weighing about twenty pounds, and composed almost entirely of solid matter. Its greatest length is 18 inches; breadth, 12 inches; thickness, 7·8 inches. For purposes of description it may be divided into an anterior and posterior surface, a right and left side, and an upper and lower extremity; but it must be remembered that its position before removal was oblique, the posterior surface being turned considerably to the left side of the body.

"The posterior surface is comparatively smooth and flat, of a dull grey colour, marked here and there with crimson traces of inflammation. It is entirely invested with peritoneum, and through that layer shines a fibrous tissue which, in the centre of the tumour, forms a dense net-work, but towards the superior extremity is arranged in open meshes, some of them one inch in diameter. Inferiorly the surface loses the glistening appearance which its upper portion presents, and is of that dull grey colour so characteristic of the presence of adipose tissue.

"The general shape of the posterior surface is that of the body of a guitar; and from the narrow constricted part hangs, on each side, a semi-detached tumour, that on the right side being about the size of a small orange; that on the left four or five times as large, and more irregular in shape. Immediately above the irregular tumour is to be found the pavillion of the Fallopian tube (left), which runs downwards a distance of 6½ inches, and is lost on the surface of the tumour—at least it was impossible to follow it farther. Just before breaking up into its terminal fimbriæ the Fallopian tube dilates into a transparent cyst the size of a small bean; this cyst does not communicate with nor obstruct the canal of the tube.

"The anterior surface, which looks also somewhat to the right,

is much more irregular than the posterior, and is covered with several layers of false membrane, which are deeply injected, and in some places quite black. Inferiorly, and a little to the left, is the spot at which the tumour was amputated—an irregular surface, consisting of two circular facets, about $3\frac{1}{2}$ inches in diameter, joined by a bridge 2 inches long by $\frac{3}{4}$ broad. Below, and to the outer side of the cut surface, is found the left ovarian ligament, about 3 inches long, and terminating, without any well-marked line of separation, in an ovary, which, although flattened and drawn out, appears normal, and contains a corpus luteum. The superior extremity of the tumour is convex, and consists of a large cyst whose contents have been evacuated. The inferior extremity is rounded off, and presents nothing worthy of remark.

"*Structure of the Tumour.*—On making a longitudinal section the tumour was found to consist of fibrous tissue, arranged in different fashions and in different states of perfection, and split up by little cavities of various sizes, containing serum more or less transparent. In some places the fibrous tissue was arranged in concentric lamellæ, and it was then possible to isolate, by the fingers alone, little masses varying in size from that of a nut to that of an apple, and resembling much the round fibroids of the uterus. These little masses, however, were never removed entirely whole; their connexion with the surrounding tissue was much more intimate than it is usually in an uterine fibroid. In other places the fibres were interwoven without definite arrangement; towards the inferior extremity they seemed to be in process of fatty degeneration, and in several places little calcareous masses, without any well-defined structure, were discovered. Just at the inferior extremity the surface was rendered irregular by some little hard nodules, which, on being cut into, presented the appearance of tubercle. The solid tissue was everywhere permeated by large blood-vessels, and in several places blood-cysts, the size of a barleycorn to that of a pea, were demonstrated. The largest cyst was at the superior extremity; it was about the size of an adult head, and its internal surface presented traces of having primarily been divided into several compartments. The thinnest part of the cyst wall was $\frac{1}{4}$ inch; its lining was smooth and glistening, having much the appearance of a serous layer. At one point there projected into the cavity a yellow nodule the size of a bean, and spherical in shape. With a little trouble this nodule was taken away entire, and found to consist of a smooth investing fibrous capsule, and contents which were partly granular partly oil

globules. From the large cyst a little passage, through which might be passed a common lead pencil, led down into the little tumour which was described as attached to the right side of the tumour. The corresponding one on the opposite side was also hollowed, but had no communication with the large cyst. The cysts contained in the solid substance of the tumour were of various sizes, from a mere trace to several inches in length; their lining membrane could not be separated from the surrounding tissue; some of them communicated together by means of slender tubules."

The body was examined after death by Dr. Gordon, and the following is a description of the tumour which we did not attempt to remove:—It consisted partly of a cyst and partly of a fibro-cystic tumour. The cyst was spherical, about a foot in diameter, empty (its contents having escaped through a smooth-margined opening, an inch in length), and it adhered to the anterior abdominal wall; with this exception, the whole surface of the cyst was free and unattached, except inferiorly. The walls were extremely thin superiorly, so that at first sight they appeared to consist exclusively of peritoneum, marked, however, by the course of large vessels. Inferiorly the cyst wall was capable of being split up into three layers, the outer serous, the middle one apparently muscular, and the internal one epitheliated. A little to the right, and inferiorly, the tumour was attached by a pedicle, $3\frac{1}{2}$ inches long, 2 inches broad, and $\frac{3}{4}$-inch thick.

This pedicle was covered with a serous layer, and consisted of fibrous tissue, hollowed out here and there into little cysts, similar to those described as having been seen in the tumour removed by operation. The extremity of the pedicle had been connected with the anterior portion of the solid tumour, but was detached by the weight of the latter, when being taken out of the body. The cyst, at the operation, was seen collapsed, and lay immediately above the transverse colon, in front of which the pedicle descended. The inferior border of the cyst was further attached to the transverse colon by strong adhesions, in which were found several large blood-vessels and some lymphatic glands; two of the latter being enlarged and infiltrated with tubercle.

A part of the omentum was attached to the colon, and in it the veins were enormously distended and much convoluted. They were full of air, and resembled rather the small intestines of a fowl or of a rabbit than the blood-vessels of a human being.

On examining the uterus and the enormous fibro-cystic tumour

which was springing from its fundus, the vaginal portion of the uterus was found to be altogether atrophied—the vagina terminating in a virginal os uteri; and the sensation conveyed to the finger was that of a very light movable uterus. On looking for the body of womb, its place was found to be occupied by a long flexible tube, crackling under pressure, like thick parchment. From the upper, somewhat dilated, extremity of this tube, sprang the right Fallopian tube and the right ovarian ligament. This was in normal relation to the right ovary, which also appeared healthy. The vagina and the elongated uterus were now slit open, and the length of the entire cavity of the womb was found to be 7 inches, that of the cervix alone $3\frac{3}{4}$ inches. The greatest width of the uterine cavity was close to the fundus, but did not exceed $\frac{3}{4}$ of an inch. The left Fallopian tube had been cut through half an inch from its uterine extremity.

The walls of the uterus, like the Fallopian tube, were of normal thickness. From the fundus sprang a fibrous column, 5 inches long, 3 inches deep, and $1\frac{1}{2}$ inch broad, encircled at its upper extremity by a ligature. The left side of this fibrous column presented a roughly cut surface, 5 inches long and 3 inches broad or deep, being the point which the tumour first described had been cut through at the operation. The tumour which was left was an enormous mass, 18 inches in length, 16 inches in breadth, and near its centre fully 7 inches thick. The posterior surface was of a greenish colour, from commencing decomposition, and its smooth external glistening tunic here and there raised by rounded projections, of all sizes, from the head of a pin to that of a child. Some of these projections evidently contained fluids, others were hard, and their fibrous nature sufficiently apparent without the aid of dissection.

Here and there were traces of adhesions. On the anterior surface the walls were intensely congested with occasional rounded projections. The lower two-thirds of the tumour were, however, separated by a deep sulcus from its upper third, so that the two bodies appeared distinctly separate. The upper tumour was 11 inches broad by 6 inches long, and 6 inches in depth; its general shape strongly suggestive of an enlarged liver. In structure the tumour was precisely similar to the one removed by operation, and described by Dr. Ritchie.

Remarks.—One of the soundest objections raised to the admission of ovariotomy among those surgical operations which have been

generally looked upon as "legitimate"—(or, in other words, which it is the duty of the surgeon to perform if he is called upon to try and save the life of a patient when threatened by a dangerous disease)—is the difficulty of diagnosis. And the supporters of the operation, while they assert that many of the mistakes which have brought discredit on surgery ought not to be repeated at the present day—and can only be repeated by the ignorant or the careless—admit that, in some rare cases, it must be almost impossible to arrive at a perfectly correct diagnosis before the commencement of the operation. Still advancing knowledge makes such exceptions rarer and rarer; and it seems probable that even such cases as that described, will soon be eliminated from the list of those in which an exploratory incision must be made, or the operation of ovariotomy commenced, before the surgeon can be positively sure as to the precise nature of the growth with which he has to deal.

In the fourteenth volume of the *Transactions of the Pathological Society of London*, page 204, may be found a short account of a fibro-cystic tumour of the uterus which I removed from a single lady, aged 53, on the 30th of April, 1863. One large cyst had held 26 pints of fluid and 4 pounds of fibrine; and a solid mass, which weighed more than 16 pounds, resembled very closely the mass just described by Dr. Ritchie. The patient sank, from shock, four hours after operation, although the tumour was completely removed; and there was so little difference in the pedicle from that often seen in ovariotomy, that it was not until after *post mortem* examination that the true nature of the case was discovered. Given, a large semi-solid tumour, fluctuating in some parts, containing cysts holding upwards of 20 pints of fluid, moving beneath the abdominal wall, the uterus being movable, and not enlarged so far as measurement by the sound can detect, no sound or arterial impulse to be heard which is not often heard in ovarian tumours, and no history of hemorrhage leading to a suspicion of uterine disease—and it will be admitted that these characters of the two fibro-cystic tumours of the uterus which I removed, so closely resemble those of semi-solid ovarian tumours, that diagnosis must be very uncertain. Even after an exploratory incision, I know of nothing but a rather darker—less pearly blue—aspect of the tumour which would put the surgeon on his guard. In any doubtful case it would be well to tap the largest cyst and examine the fluid. In both my cases this was peculiar—not the viscid mucoid fluid of multilocular ovarian cysts, but a thin serum, with 5, 10, or 15 per cent. of blood intimately

mixed with it, and not separating until after standing for some hours. In this way I have satisfied myself, in at least four cases, that tumours, which others considered to be ovarian, were really fibro-cystic uterine growths. If the operation has been commenced, and the dark aspect of the tumour is observed, it would certainly be advisable not to do more than tap one or more of the largest cysts before examining attentively the connexions between the uterus and the tumour. If these should prove to be very intimate, it will be the unpleasant duty of the surgeon to desist from any attempt to do more, and to close the wound as soon as possible. At least, the two cases in which I made the attempt to do more, have convinced me that I tried to do more than can be done with a fair and reasonable hope of saving life.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Three Hundred Consultations in Midwifery. By Robert Lee, M.D., F.R.S. London: Churchill and Sons, New Burlington-street. 1864. Pp. 217.

Our task is over. We have carefully read this volume; and we entertain considerable doubt if amongst its records can be found an instance of labour more tedious or irksome than that which we have undergone in completing its perusal.

This is a small book, comprising a number of meagre notes of cases in obstetric consulting practice, thrown together without classification, and unembellished by any practical observations. "After the manner adopted by Paul Portal, Mauriceau, Gifford, and Smellie," to use our author's own words, have these three hundred cases been collected, that is, in chronological order. Indeed, one would fancy Dr. Lee endeavoured, as far as he could with decency, to keep himself within the systems of practice adopted by these worthies. That there are few novelties to be found in this collection it is needless to say, but it abounds with the peculiarities of Robert Lee. Though his remarks upon these cases are extremely few, they are sufficient to acquaint any reader (even a stranger to Dr. Lee's writings, and ignorant of his peculiar views) with the tenor of the author's mind, namely, that he is averse to all deviation from the old and beaten track, except it be upon his own recommendation; that he is unsparing in his sarcasms against everyone whom he deems an innovator; and that he exhibits an undignified, as well as an ungenerous intolerance against those who entertain different opinions from himself.

This little volume, though very defective in many respects, is by no means without its use. It is a quaint record of good and bad practice, both on the author's own part and upon that of his consultors. One lays it down, moreover, with a sigh of relief as well as of sadness

after having read such numerous disclosures of *total* ignorance of obstetric medicine, as exhibited in the copied letters and detailed actions of persons, who have undertaken the care and assistance of woman in her hour of trial. The narration of cases of malpraxis are useful as warnings to others, while the exposition of pretenders may render them less daring, and therefore less injurious to the public. It must not be supposed that our author gives the names of these latter, but he details such cases, in a manner so peculiar, as shall leave no doubt in the minds of each distinguished member of the profession as to his connexion with the case.

We shall detail a few instances such as we have alluded to:—At ten o'clock one Friday (whether morning or evening is not stated) the author was called to see a case of labour, which had commenced on the previous Wednesday afternoon. The medical (?) attendant informed him "that the *urinary bladder had descended before the head;* that the head was firmly impacted in the brim; and that the brim was distorted. Dr. Lee, upon examination, found "the *membranes protruding through the external parts; the urinary bladder in its natural situation;* the patient not exhausted; pains strong; *head descending into the pelvis, and the pelvis of the usual size.*" Our author gave this person the following impressive lesson:—He "begged Mr. —— to push his finger through the membranes and let the liquor amnii escape; and to introduce the catheter into the bladder" (Case 96). Here we have an example of absolute ignorance of the most rudimentary department of practical midwifery. It is more than probable that this person had never attended a labour case before, nor had even been present at an institution to see one attended by another. This "attendant" does not exhibit even a "little knowledge," and probably so much the better was it for his patient; for, wanting the self-confidence arising from the impudence inseparable from such a condition of obstetric endowment, he is not above obtaining the advice of Dr. Lee, who gives him a most admirable and practical clinical lecture. It is true the poor patient's pain might have been curtailed, but fortunately no danger of any consequence could have arisen while the membranes remained so long entire with plenty of liquor amnii. The next case we shall quote offers a different type as regards the professional acquirements of obstetric physicians. This attendant informs Dr. Lee that labour had commenced in the afternoon; that there had been a great quantity of liquor amnii, and when he examined he felt a hand, but that afterwards the breech presented

Two hours before Dr. Lee's arrival the trunk and extremities had been expelled, but "the head was remarkably large," and "could not be extracted." "I found," says Dr. Lee, "the trunk and extremities hanging externally, and the child cold and dead. I took off my coat, turned up the sleeves of my shirt, made an examination, and found the head in the pelvis, in the most favourable position for extracting," which extraction he readily accomplished (Case 235). Now in this case the ignorance exemplified is not so great as that in the one above. The attendant here may or may not have mistaken a foot for a hand; at any rate, he was aware that it was subsequently a breech presentation, and had, in short, a better knowledge of his subject; yet how infinitely more disastrous did this case prove than the former. In this, notwithstanding the knowledge, the life of a child was lost; in the other, as we have seen, nothing unpleasant occurred. In one case self-sufficiency proves disastrous; in the other complete inefficiency proves a safeguard. It may be here observed that Dr. Lee always informs us when he takes off his coat, and turns up his shirt sleeves; but omits to mention whether he folds the former garment and places it neatly on a chair, or throws it carelessly on the bed. As he *does* tell us so much concerning his garments, we see no reason why he should not complete that department of the operation.

Matters improve as we proceed—*i. e.* an increase of obstetrical wisdom going *pari passu* with increased boldness on the part of the attendant and increased probability of serious results to the mother, The following letter is sent to Dr. Lee:—"I shall feel obliged if you will come to me at the above address, and bring your forceps, &c." (The *et cetera* means crotchet and perforator.) "I have been here twenty-eight hours, and *have failed to deliver the head by means of my* OWN *forceps.*" This lady is forty-two years of age, and in her first labour. "I suppose," says Mr. —— to our author, "it is a case of twins, as the pains do not help us at all. *The head is quite low down.* I have tried to move it with the forceps, but I *cannot make the forceps lock.*" On Dr. Lee's arrival he found this patient with a good countenance, not a bad pulse, not much exhausted, but with inefficient uterine action. But the tongue was furred, and the vagina and external parts greatly swollen and intensely red; and last, not least, the head of the child *had not passed the brim of the pelvis.* This accoucheur had advanced to a forceps; he had one of his own, and he had clapped it on three times in the case just alluded to. He spoke learnedly also to

his consultant, and suggested the existence of twins, because the pains were inefficient. Though the head was low down "quite," he advised the long forceps. Of course, as he had tried three times with his own—a very short one we hope—and failed, it would have been superfluous for Lee to have attempted with his short one. But what was the result of this case? Not only had the head not passed the brim, but it was likewise "much compressed; great swelling of the scalp, the head beyond the reach of the forceps." "The first question I said," continues Dr. Lee, "for our consideration is, can the patient be left longer in labour in safety? If she is not delivered soon will some serious mischief ensue? We were agreed on these points, that labour could not be allowed to continue longer. I thought the head too high up and too large to allow of the forceps without great danger to the mother, and no chance of saving the child I expressed a decided opinion that the only means of rescuing the patient from the dangerous state was to deliver with the perforator and crotchet. Mr. ——, who had adopted the new opinion, that craniotomy ought to be banished altogether from midwifery, would not agree to this, but insisted that the long forceps should be tried. I suggested that Dr. —— should be called into consultation. Dr. —— said this is not a forceps case. The only thing that can be done is to open and extract the head. The child must be sacrificed. We do not know that it is alive. The probability is that it is dead. Meconium is passing. I opened the head, and great and long-continued force was required to extract it with the crotchet. Great force was required to draw the shoulders through the pelvis; and though Mr. —— supported the perineum carefully, it was, most unfortunately, lacerated to some extent, and ever since has been of great inconvenience from the injury done to the parts, a part of the sphincter ani having been torn. The patient was in an almost incoherent state during the whole time I was engaged in delivering the child, and afterwards she was seized with puerperal mania, from which she has slowly recovered" (Case 252). This woman had been left in hours of unnecessary and unavailing suffering through sheer ignorance. There was narrowing, or at least disproportion of some kind—head or pelvis being at fault. The fetal cranium was wedged in the brim, unacted upon by the pains; and a forceps was used several times by an individual who was incapable of judging as to the real nature of the case.

We could quote upwards of forty such instances of defective education, exemplified in this small volume, did our space permit,

but shall notice but two more only. One in which the attendant becomes much alarmed, "because he could not feel the uterus, and was conscious of the existence of some tumour, which turned out to be the placenta coming through the os uteri, but slightly caught in its passage by the cervix, and capable of being removed without difficulty." The other it is necessary to give *in extenso*, and in our author's own words:—

"Case 260.—At 9, p. m., on Saturday, the 22nd of September, 1860, Mr. —— called upon me, and requested me to accompany him to the house of a patient who was in the utmost danger, in consequence of a large portion of the placenta having been left within the uterus eight days after delivery. He had not attended the patient during the labour. Being accidentally in London, and having had the care of the patient some years before in the country, and being informed by her husband of the alarming state in which she was, he had gone to see her, and, in consequence, had requested that I should be consulted. Being entreated likewise by the husband, who accompanied Mr. —— to my house, to go, and, if possible, save the life of his wife, I could not refuse to do so. I found the patient with a rapid pulse, 140; the abdomen tympanitic, but not tender on pressure. Tongue furred; no vomiting. The whole atmosphere of the apartment tainted with the fetor of the discharge. I made an examination, and *felt the upper part of the vagina filled with a large mass of placenta, a portion of which had not cleared the os uteri.* With the fore and middle fingers of the right hand this was removed with some difficulty. It was in a condition not to be described—horribly putrid. The windows of the room were thrown open, and the corrupted mass disposed of as quickly as possible. I requested that the vagina should be washed out thoroughly with warm water; that this should be done at short intervals; and that some aperient medicine should immediately be administered. The patient recovered favourably. Before leaving the house I could not avoid expressing to the husband my disapprobation of the treatment which had been pursued by the practitioner to whom he had entrusted the care of his wife. My opinion being demanded, I had no choice but to discharge my duty on this occasion, as I have done on all occasions when consulted. The following letter, containing the details of the case, was addressed to me, September 27th, 1860, by a gentleman who had seen the patient on the same day that I was called to remove the placenta, but whose name had not been mentioned to me:—

"I take an early opportunity of setting you right upon one or two points in regard to the case of retained placenta, to which you were called last Saturday evening. I must tell you I was called to the patient at 12 o'clock, midnight, on Saturday last, the day on which you saw her, and she was delivered into my care by the gentleman who was engaged to attend her, and who was compelled to leave for Brighton—(the gentleman who actually attended her I did not see). I found, at my visit, that a full examination *had been made a couple of hours before*, and that the *vagina was positively free from clots*, &c.;[a] there was no hemorrhage; the os was open more than usual. Under these circumstances I did not feel justified in examining then; but I ordered several doses of ergot to be given, with a view to expel any remaining mass, and I *advised* to defer *the examination, if hemorrhage occurred, till my evening visit.* In the mean time, a Mr. —— arrived from ——, and I saw him, and he coincided in my treatment. I left him at 8, to inject a stream of tepid water into the uterus; but, at 8.30, he took upon himself to disregard my interest in the case, and to send for you; *and you arrived and forestalled me in removing the placenta's remnant, which my ergot had expelled into the vagina.* You were not told that any ergot had been given. I mention these facts in order that you may rectify your notes of the case now, and not leave to ——— in the event of the case appearing in print at any future time. I must say that Mr. —— has been guilty of a most glaring breach of professional etiquette; and it was unhandsome also on your part that you should have disregarded me, when you were perfectly aware that I had sole charge of the case, and you were wholly unjustified in making the remarks you did. I hope, however, that I may be mistaken, and that it arose from a misconception on your part.—I am, etc."

"On the 28th September," continues Dr. Lee, "I addressed the following note to the writer of this letter:—'Will you inform me who it was who actually attended Mrs. ——, and when the confinement took place; and, likewise, who it was who had made a full examination a couple of hours before you saw the patient?' The following reply was received:—'It was the eighth day after confinement on which you saw the patient. As to the antecedents of the case, and treatment, I cannot speak; and, indeed, as the case was mine only for a time by accident, I should not feel justified in

[a] The italics are our own.

furnishing any particulars beyond those I have. P.S. My object in writing to you is, chiefly, to defend my own treatment.' These letters were sent to the gentleman in the country, by whom I had been called to see the patient. *It must be obvious to all who peruse* this correspondence, *that it is difficult to obtain full and accurate histories of cases in midwifery*, etc. All who reflect must be convinced that principles, as they are called, or doctrines which do not rest upon a large number of faithfully recorded cases, can only lead to pernicious errors in practice." The following is the letter received by Dr. Lee from the gentleman who had called him to see the case:—"I felt very sorry, on my return home, on Friday evening, that I had not time to answer your kind note, as the post was just closing. It is evident that Dr. —— is much disappointed he cannot appropriate any credit in the treatment of Mrs. ——. I congratulate myself, and the friends of Mrs. —— are deeply gratified, that I called you to see her. It is my firm conviction that had she been left another day without proper management, the case would have terminated fatally. I believe I told you what occured upon my arrival; however, I will briefly recapitulate what I said to you. I found the mother and husband of my patient much distressed, and foreboding the worst results, and well they might do so; for Mrs. —— was talking incoherently, the pulse 160, the abdomen tympanitic, and there was a total absence of mammary secretions. After a short time, Dr. —— came, and informed me what he had done and prescribed; that an examination had been made, and the vagina and os uteri found perfectly free of coagula and debris of placenta; that he was giving a mixture, with dilute sulph. and liq. secale (of which about two or three doses had been taken) to *counteract any hemorrhagic tendency. Not a word was said by him that any portion of the placenta was yet remaining in the uterus, nor did he say that he should make an examination in the evening.* I suggested an injection of some disinfecting liquid into the vagina, and also into the uterus, as the discharge was still of an offensive character; and, as a medicine, carb. ammon. and camph. mixture, with lemon-juice—the former in excess; *but, before doing anything, I should call in some leading physician, as I considered the case pregnant with danger.* Dr. —— made no objection. Before we reached Mrs. ——'s house, I hinted to you that we ought to acquaint Dr. ——; but it was thought not convenient to detain you until he was sent for, *especially as he had told me he was making alterations in his house, and might not be at home.* Not a word was said by me of any remark made by

yourself, only that you had removed a large mass of putrid and stinking placenta. I explained to him how it happened we could not see him, and I thought he seemed satisfied. I am at a loss to imagine how he could charge me with being guilty of a most glaring 'BREECH' (*sic*) of professional etiquette. I suppose he may be much surprised that a country practitioner could not be perfectly satisfied with his opinion; for he told me that he had 300 such cases as that of Mrs. ——. He evidently took it for puerperal fever, as he said *he was about to publish on that disease*, and that he had just read a paper upon that subject before the Obstetrical Society; and that Dr. ——, the previous attendant, had declined to see Mrs. ——, fearing that he might communicate the complaint to a lady to whom he was hourly expecting to be called. I shall not notice his remarks," &c., &c. (Case 260.)

Need we apologize for giving this case *in extenso*, exactly as it is given to us by our author? In connexion with the case immediately preceding, it is most instructive; for, when contrasted, they again afford the extremes of the two leading types of ignorance met with in obstetric practice. The former exhibits complete ignorance in the practitioner; who, though he has the temerity to attend obstetric cases, yet is perfectly ready to permit his inexperience to be discovered. The latter is an example of one possessed of an empty smattering of his subject, together with boundless impudence and assumption—added to which, an amount of cunning calculated to gloss over error. The first is by no means so dangerous a person as the second. The one, conscience-stricken, at once cries out, "God be merciful to me a sinner;" he calls for aid on one who can remove him from his predicament, and thus prevents the possibility of evil results; while the other acknowledges no error—admits of no superior—but brazens it out with unblushing effrontery; and, even though he must stand self-convicted, acts his part to the end. This person, of course, never requires assistance, and so one need scarcely feel surprised that evil results should have thickly followed on his trail. Thus, though we by no means potently believe all he says, when he informs "the young man from the country" what a swell he is; how he contemplates authorship, and how he gave some hints to the Obstetrical Society of London; yet we must admit that his crowning assertion, as to the 300 cases of puerperal fever he had seen in his practice, may have had some of the element of truth in it, in this way—viz., that the amount of puerperal complications in his practice bore an enormous ratio to the number of his deliveries. We

trust this gentleman, if he has published, has committed as few "breeches" of the rules of orthography as possible. Certainly, after the perusal of this history, we must concur with our author that sometimes, at least, "it *is* difficult to obtain full and accurate histories of cases in midwifery."

We feel convinced that instances of malpraxis and exhibitions of professional incompetency have been of more frequent occurrence in obstetric medicine than in any other branch of our profession. Yet one would have supposed that, apart from the interests of humanity, students would for their own profit as well as for their peace of mind—while yet there was time—have endeavoured to procure a sound practical knowledge of this subject; such, however, has not been the case heretofore. Perhaps to those, some time since, engaged in medical education, may be attached a portion of the blame for this melancholy condition of affairs. During our own studentship we remember it was much the fashion of the schools to decry midwifery. Ignorant of obstetric medicine themselves, from the irksomeness attending the acquirement of a competent knowledge therein, some endeavoured to promulgate the idea that it was an unimportant branch (if a branch at all) of medicine. We have often heard the greatest ridicule thrown upon the subject, and that in language by no means refined; while its practitioners were, even then, viewed with some degree of contempt. Such having been the case we need scarcely be surprised if obstetric medicine was then by no means a favourite study with the student. Attendance on lectures and cases, of course, were necessary to qualify for examination; but this was done *pro formâ*, and slurred over as quickly as possible. The old system has now most happily been removed to some extent, and the great, the paramount importance of midwifery is beginning to be appreciated. It has been found that the public require competency as well as attendance in this branch of medicine, from the *great majority* of practitioners; and so the term "midwife" as applied to such in contempt, together with the sneers and sarcasms once so profusely and loudly flung at them are now but sparsely discharged and faintly expressed. In our school of medicine the study of midwifery has, for a considerable period, occupied its due share of attention; and hence it is, that, with us, there are not observed such frequent and lamentable specimens of malpraxis as stud so thickly the pages of this small volume.

Concerning the practice of our author we regret to state, that in

many respects it appears by no means an improvement upon that of the majority of his consultees. We are at issue with him upon almost every point, and in none so much so as in his treatment of tedious and difficult labours. We consider him to be decidedly too fond of his perforator and crotchet. Indeed, sometimes of his crotchet *without* his perforator. He is peculiarly hostile to the forceps, if one may judge from the perusal of this volume at large; and seems to revel in delight when he meets with a case of lacerated perineum which he can, by any means in his power, connect with that instrument. Doubtless the forceps may be much abused. And from the records of this volume, exhibiting, as they do, so many examples of incompetency to practice obstetric medicine, one must arrive at the conclusion that it *was* frequently used unnecessarily as well as clumsily in those instances which came under the author's observation. But granting such to have been the case, there is, on that account, no reason to ignore, to such an extent, the use of so valuable an aid. Surely in the hands of so experienced a practitioner as himself, Dr. Lee could not have apprehended such intense injury and danger from its use; even though he *did* lacerate a perineum, by dragging with the crotchet, a lessened head through the vulva (*vide* case 190).

Case 92 is an excellent example of the timidity exhibited by our author with regard to the use of this instrument. A young woman is, from 7, p.m., to 12, progressing in the second stage of labour; but from 12 to 8, a.m., matters remain stationary. The author is called in, and recommends waiting "a *few* hours" longer, in the hope that the child would be naturally expelled. He does not tell us if the case was one of simple inertia, and whether any means were adopted, or recommended to stimulate the uterus (which Dr. Lee must know may be done without his friend (?) the ergot); but he merely *waits* four hours longer. The result is, *exhaustion* takes place, and then the forceps are applied, and not *till* then. We are not informed if this woman's convalescence was delayed—our author is often very curt when it suits—but, surely there would have been less *chance* of prolonged convalescence if *exhaustion* had not been permitted to supervene. We presume that this was a case of simple want of efficient uterine action; for otherwise, sufficient power would have been exercised upon the head to cause considerable pressure on the soft parts (against which it had been imposed for so many hours), and to have given rise to some subsequent inflammatory action in those parts. Had such occurred, it would

most certainly have been mentioned with grim satisfaction; and the forceps would have incurred the blame, which should have been attached to the author's want of decision.

In illustration still further of this timidity in the use of the forceps we shall draw attention to case 87; and here we shall have a more extensive view of our author's bad practice than in the former case. We shall, however, give the history in his own words:—

"On the 9th of November, 1851, I was attending Lady —— in her first labour; the presentation was natural, *and there was every prospect that in a very short period the process would be happily completed.* Suddenly a *great extravasation* of blood took place into one of the labiæ, and it became swollen and livid, and *actually burst* on the inner surface, and a large effusion of blood took place. Mr. —— was requested to see the patient in consultation, and we agreed that immediate delivery was necessary, and the forceps could not be applied, although *the head was within the pelvis;* turning was out of the question. *I opened and extracted the head very speedily;* recovery was in all respects satisfactory."[a]

The first conclusion one arrives at after reading the above is, that our author was unacquainted with the nature of a labial thrombus; for he informs us, that from the great extravasation of blood, the labiæ "*actually*" ruptured "on the inner surface;" as if such an occurrence was to him a matter of surprise. But after the extension of the labiæ—which must have been great, as the extravasation was "great"—had been relieved by the rupture, and as there had been previously every reason to suppose that the process of labour "would be happily completed," and as the head was "*in* the pelvis," what could possibly have contra-indicated the use of the forceps? Was it through fear of lacerating those already distended parts? Was it because an ear could not be felt? How did our author arrive at the conclusion that "the forceps could not be applied?" Did he try? Was it possible that labour might have been even naturally completed, notwithstanding the ruptured thrombus? We have seen such instances, although the hemorrhage had been profuse and the thrombus very large. Why did he give no chance whatever to the child? We have no hesitation in stigmatizing this as an example of grossly bad practice. After this piece of teaching to the students of St. George's Hospital, our author had better

[a] The italics in this case are our own.

restrain his sneers against the "school where" he has "every reason to believe unsound doctrines are taught, and where the teacher has derived his knowledge chiefly, if not entirely, from German and French books" (p. 171).

We shall allude to but one more case (127), as illustrating the practice of Dr. Lee in cases demanding instrumental interference. We could, however, adduce many in which are disseminated as pernicious doctrines as ever emanated from any school, whether German or French, or even that of St. George's Hospital. This we shall also copy *in extenso*:—

"On the 3rd of February, 1854, Dr. —— requested me to see a patient in Stanhope-street, Regent's Park, who had been upwards of *thirty hours* in labour with her first child. *The pains had almost entirely ceased, and there was great exhaustion.* The head, *much swollen*, was *firmly impacted* in the brim of the pelvis. An ear could not be felt. We waited *eight hours* to *see* if *the pains would return*, but they ceased completely. And it being obvious that the head would never be expelled by the natural efforts, we resolved to deliver by craniotomy. In removing the placenta, which was retained within the uterus beyond the usual period, I ascertained that the base of the sacrum projected forward unusually. The hemorrhage and faintness which followed ceased, and the patient recovered most favourably. After the labour had been happily completed, Dr. —— expressed his surprise how any practitioner in midwifery could venture to apply the forceps to the head firmly impacted in the brim of the pelvis, *when there was no room for the blades to be introduced.* I stated, as the result of my experience, that fatal contusion of the soft parts was often the result of this injudicious proceeding, sloughing and vesico-vaginal fistula."

Now let us for an instant place ourselves in the situation of one of the pupils of St. George's Hospital, and endeavour to enlighten ourselves, according to the doctrines to be gleaned from this case.

It follows, then, that after the endurance of thirty hours' labour, when the head is closely impacted in the brim, we must wait eight hours longer "to see" if the pains will return, notwithstanding our patient suffers from "great exhaustion." In a case such as the one just narrated, it does not seem that a very great amount of acumen was necessary to arrive at the most correct line of proceeding. Here is perfect impaction at the brim; there is not room enough even for the thin blade of a forceps to be insinuated between the fetal head and the mother's pelvis; the latter lies in a state of great exhaustion

after having endured thirty hours' torture. Why wait eight hours longer "to see" if pains will return, which, even did they, might not suffice to move the presentation one line further. Or if, after completing, by some marvel, the delivery naturally, would, most certainly, have produced a dead child—which might have been dead already for what we know—no mention having been made that auscultation was practised during the progress of the labour. In this latter respect we here take the opportunity of stating that our author's histories are very defective. He should, in each case of arduous labour, have stated the results of auscultation, or whether this aid to our action in such cases had been omitted. He very rarely does so. In the above case we presume it was not had recourse to, for had it been practised with care during these thirty-eight hours; and had the heart's sounds been carefully noted, there might have been *less necessity for waiting eight hours for the return of pain.* We also take this opportunity of informing Dr. Lee that in the use of the stethoscope we have a very certain means, as a general rule, "by which the life or death of the child" can "be determined with certainty in cases of difficult labour;" and that he may derive from it that "most important assistance in the treatment of cases of protracted labour." We mention this, as, from his observations at page 131, we suppose him to be ignorant of the practice of auscultation in obstetrics.

We disagree completely with Dr. Lee as to his views, both as regards the use of the forceps and that of the perforator in tedious and difficult labours; and we perfectly concur in the assertion of our contemporary, the *Lancet*, "that if, in 171 cases of difficult labour, we must be called upon to perform craniotomy in upwards of 90, the practice of midwifery should ever afterwards be avoided by us."—(*Lancet*, Nov. 5, 1864.)

We now pass to the treatment of placenta previa, as presenting some novelty, upon which we have one or two observations to offer.

The method adopted by our author in the treatment of these cases is as follows—so far, at least, as we can glean from his scattered cases. Whether it be full time or not, once the placenta can be distinguished to occupy a situation over the os and cervix uteri and hemorrhage occurs, delivery must be completed in the following manner:—By introducing the hand into the vagina, getting two fingers (one at a time) through the os uteri, pressing aside the misplaced placenta, searching with the two fingers for the foot, and with them drawing the extremity of the fetus through the os, and

completely delivering—in other words, forced delivery. This practice he frequently adopts also, in cases of partial placenta previa. The *tampon* he barely mentions, save as a remedy in another form of hemorrhage, and to which we shall presently draw attention.

We have collected from our author's records thirty-one cases of this complication; eight partial placenta presentations, nineteen complete, and four in which the particulars or the amount of the attachment over the cervix are not stated. Of these we shall give a brief summary:—Of the eight partial placenta presentations four were treated by forced version; two by version, the os being favourably disposed for that operation; one by rupturing membranes; and one by rupturing membranes and then craniotomy. Of the four treated by forced version three mothers recovered—what became of the other is not stated—and one child was dead-born; the others are unaccounted for. Of the two treated by ordinary version one mother recovered, one is not accounted for; and what became of the two children is not mentioned. The case treated as accidental hemorrhage recovered, but the child was dead-born; and the mother recovered who was treated by rupturing the membranes and delivering by perforator and crotchet.

The nineteen complete placenta presentations are thus disposed of. Forced version was practised in eleven cases, three mothers died; two children were born alive, and one breathed for a little; and what became of the four other children has not been stated.

In five cases the os was favourably disposed for version, and one mother died, three recovered, and one is unaccounted for. There is nothing said of the children. In one case the placenta was pierced, the waters let off, and labour was finished naturally; the mother recovered and the child died.

In one case the placenta was pierced by the hand and version performed; the mother recovered, and the child was dead-born.

Of the four cases in which the amount of attachment of the placenta over the cervix is not mentioned—two were delivered by forced version; both mothers recovered, one child was lost, and we have no account of the other; one mother was delivered while dying; both lost; and one was delivered, the os being favourable for that operation, and was saved, as was her child.

The results of our author's practice in placenta previa, taking his cases indiscriminately, we find to be as follows:—

Of the 31 mothers, concerning whose cases he was called on to consult, 22 recovered, 1 we can only suppose to have recovered, 5

died, and we hear nothing of what became of 3. The 31 children resulting from these deliveries are thus disposed of:—4 were born alive, 1 breathed for a little, 11 were dead born (12 died), and of 15 we have no account.

Omitting those mothers who have not been accounted for, we have 28 and 5 deaths, or 1 in $5\frac{3}{4}$. Of the children 16 only are accounted for, and 4 mentioned as saved, or 1 in 4.

These statistics of all the varieties of this complication, are very imperfect, and therefore we cannot arrive at any very definite or satisfactory conclusion. We find that the mortality to mothers in cases of complete placenta previa was 1 in 4, whose labours were completed by forced version. This practice was adopted in the partial variety but four times, and none of the mothers were lost.

It is to be regretted we cannot form an opinion of this mode of proceeding; but we are constrained to believe that the omissions as to the result of cases would not have been so numerous, especially in respect of the children, had it been very successful. We are led to form this idea, from such bitter flashes of exultation as the following:—After detailing an operation of forced version in placental presentation, in which the child was fortunately saved, the author concludes thus:—"What would the result have been had the placenta been torn away, and the child left to take care of itself" (!)

Dr. Lee, as we have before mentioned, ignores plugging in cases of placenta previa. In this country it has been found a most successful mode of proceeding, especially in cases coming under treatment at the commencement of the hemorrhage. Our author uses the plug, however, under other circumstances, which to us appear most reprehensible; but we shall illustrate his practice on this point by a case or two, in order that our readers may judge for themselves.

Case 74.—"On the 24th April, 1851, Mr. —— requested me to see a patient in the eighth month of pregnancy, who had suddenly been attacked with flooding about 8, a.m. A great quantity of blood escaped in a short time, and she became exceedingly faint; she had not been exposed to any accident of any kind. Great alarm was excited by the hemorrhage, for she was fully aware of the danger. Mr. —— was immediately summoned. There were no labour pains, and the os uteri was so slightly open that only one finger could be introduced, and the part was extremely rigid and undilatable. The patient was desired to remove to bed, *and vinegar*

and water were applied over the lower part of the abdomen, and cool air admitted. At 1 o'clock in the morning the flooding and faintness returned, and I saw the patient half-an-hour after. Two fingers could, with difficulty, be passed through the os uteri; and with these a portion of the placenta was felt at the anterior part of the cervix. Immediate delivery was required, but the hand could not be introduced to turn the child. With the two fingers introduced through the os uteri, the head of the child was pushed aside, one of the knees was seized, and then the foot, but it was impossible, from the contracted and rigid state of the os uteri, to draw the foot into the vagina without risk of injury. All the efforts I could make were unavailing; and it was resolved, therefore, to desist for a time, till the os uteri had become more yielding. The hemorrhage having ceased, Mr. —— remained with the patient—prepared, the instant it became possible, to seize the foot and extract the child. At 7, a.m., *the hemorrhage being renewed, with great faintness*, and the os uteri not only more open, but more dilatable, the foot and leg were drawn through by Mr. ——, then the nates, and the whole child extracted without much force. The placenta came away at the same time. A slight oozing of blood having continued, I saw the patient at 11; recommended stimulants to be given freely; ice in a bladder to be applied to the external parts, *and if the discharge continued, a large sponge to be introduced into the vagina, and that it should be pressed up firmly against the os uteri.* A binder and pad had been applied. The child was dead. The patient recovered favourably."

In this case of placenta previa, where the first gush of hemorrhage occurred, the os uteri being only just commencing to dilate, the only treatment adopted was—*vinegar and cold water to the abdomen!* Consequently, in about twenty-five hours or so, flooding returns, *with faintness;* but the os uteri will not admit of forcible dilation, and an attempt to turn is abandoned, the patient being again left to herself, in order that the os uteri may become more yielding. And so assuredly it does, for in about seven hours more the *flooding returns*, "with great faintness," and then there is no great difficulty presented against version, which is completed; and so far this woman had a most miraculous escape from death. However, some oozing continued after the placenta had come away, and Dr. Lee recommended the *vagina to be plugged*, if ice and stimulants fail in arresting it. The cold happily succeeded, the oozing ceased, and this poor creature most fortunately escaped from *a second chance of*

the grave, for she had not been plugged after the completion of her labour. The question arises: could this patient have been saved from the second and third gush of hemorrhage? We have no doubt in answering in the affirmative. Had the sponge, properly introduced, been well compressed against the os uteri after the bleeding which occurred at 8 a.m., there would have been no return, and two attacks of "*great* faintness," from loss of blood, would have been avoided; and the os uteri, being also stimulated, uterine action would have continued free from bleeding, and rendered unnecessary an abortive effort at version. True, this woman recovered; that is no reason the practice pursued was good—she would have recovered quicker and better had she not lost half the blood in her body, which would have been preserved had the case been treated properly. With regard to the utility of the recommendation of the tampon, after the completion of the third stage of labour, in order to arrest hemorrhage in that stage, there is no illustration offered in the case above; but Case 225 affords most useful information on that head. In this case our author—after the temporizing system has been adopted for some time at the expense of the vital stream—performs version during a gush of hemorrhage. Immediately after delivery another gush occurs, which does not yield to cold or pressure. (The introduction of the hand was not tried.) And so *a large sponge was introduced into the vagina*, and masses of ice applied externally—but all efforts fail; hemorrhage, of course, continued *into the uterus*, and the woman dies! Here, then, is an appropriate place for us to draw the curtain over this volume. Why enter further into the detail of its contents? Enough has been already said of it, and were we to continue our observations we should, doubtless, weary our readers, and most certainly disgust ourselves. Would that the veil we now draw over it could hide it from the world for ever. In our school it will have no effect. In that of England we have the consolation of knowing many sound and enlightened manuals and monographs are in existence, from the pens of gentlemen of more liberal minds than our author, which will counteract to a great extent the pernicious doctrines contained in this, Dr. Lee's last publication. In conclusion, we beg to remind Dr. Lee that he glories too much in finding fault with others, and we would beg to call to his recollection the old proverb:—

> "Crimina qui cernunt aliorum, non sua cernunt,
> Hi sapiunt aliis desipiuntque sibi."

On Diseases of the Throat and Windpipe, as Reflected by the Laryngoscope. A complete Manual upon their Diagnosis and Treatment. Embellished with 116 Engravings. By G. D. GIBB, M.D., M.A., &c. Second Edition. London: Churchill. 1864. Post 8vo. pp. 481.

THE book which bears this inviting title, is a very nice-looking volume of 481 pages, with a very carefully compiled index, and a glossary. It is the second edition of a treatise on diseases of the throat, published before the introduction of the laryngoscope. Dr. Gibb has long been before the medical public as an author on this and kindred subjects.

The book before us is a combination of the first edition and a pamphlet recently published, which we reviewed in a previous number of this Journal. Such a book should be a class book on the subject. The author must have extensive experience; he has evidently expended much labour upon the book, and appears to have spared no expense in illustrating the cases with woodcuts, some of which are extremely well done, the others, to say the least, are rather extraordinary. We are told in the Preface that "all the engravings, with a few exceptions, have been carefully executed by Hart, from drawings made by the author." The illustrations of the instruments are all good, but the illustrations of the cases are of the most primitive kind.

He tells us that "he has spared no effort to make his book a useful and practical work—one that could be consulted on emergencies;" and, also, that nothing has been omitted having any relation with diseases of the throat and larynx, and he states that "these diseases are a class of maladies which may be now claimed as coming especially within the province of the physician, *who can devote that patient attention and care to their investigation and treatment which surgery does not permit.*" We shall presently give our readers some specimens of the satisfactory manner in which Dr. Gibb has fulfilled his promise of providing the profession with a hand-book.

Dr. Gibb claims great things for the laryngoscope; he says, "the satisfactory results obtained in many of the cases narrated could not have been accomplished unaided by the laryngoscope. In the hands of those accustomed to its manipulation its use effects what has

been heretofore looked upon as marvellous, for it renders 'the dumb to speak and the deaf to hear.'"

The classification of diseases in the table of contents is curious; but we must not object, for the author has prepared us in his Preface:—"Objections might be taken to some of these, but the object held in view was simplicity and general convenience." We cannot exactly see either the convenience or simplicity of calling hooping-cough an exanthematous affection; or classing the the sore throat from tobacco, which he tells us in the text is caused by the smoke coming in contact with the mucous membrane, under "diseases arising from systemic changes."

We will now give our readers a short *resumé* of the chapters *seriatim*.

The first section of the first chapter is devoted to the consideration of "dysphonia clericorum." And we are told that "medical men rank next in frequency" to the clergy, and "a moderate proportion exists amongst members of the bar." We pass from the description of the symptoms—for Dr. Gibb tells us nothing new—to the treatment. This he divides into "measures of a local character, and into those of a general or constitutional nature." Under the first head he places insufflation of powders or liquids in fine vapour or spray, or directly applied by means of brushes or sponge.

His constitutional treatment he describes thus:—

"My favourite remedy is the iodureted iodide of potassium, or a weak Lugol's solution, combined with some carminative and tonic, of which the *hydrastin*, the active principle of the *hydrastes canadensis*, is one of the best." He also mentions iodide and bromide of ammonium. "In advanced cases lozenges of the *eucalyptus rostrata*, the red gum of Western Australia, are of extreme value."

"In giving these remedies attention should be paid to the secretions by the use of mercurials in alterative doses, or the use of aloes, or regulated doses of the podophyllin, the active principle of the *podophyllum peltatum*, combined with *leptandrine* and hyosciamus, or *iridin*, with mucilage, in pill, or some other laxatives.

"The inhalation of the spray of mineral waters has been found useful, also, by Drs. Sales-Girons and Trousseau; but caution is necessary in employing it, for pneumonia has resulted when the spray was impregnated with tannin." Then follow twenty-seven cases selected out of "some two hundred examples in" his "note books;" seemingly chosen more for the respectability of the patient than the interest of the case. One is a well-known Canadian

merchant, another is Rev. T. S. N., Rector of ———, at Lincoln, and so on. In not one of these cases is there any account of the course of treatment; only in two is it even mentioned, namely, Case 7 and 13. In case 7 he says, "I advised, besides other measures, the inhalation of tannin, with the pulverizing fluid apparatus of Weiss, which was procured by him, with what results I am not aware." Did the patient die of pneumonia?

The other sections treat of chronic diseases of the windpipe, diseases of the cartilages, consumption, and bronchitis, and weakness of voice. There is nothing very new in these sections, with the exception of the plates, and the author's mode of expressing himself. Fig. 7 is described as "the epiglottis, below which is seen the tumour of the right false vocal cord, shaped like a volcano with a crater." The author's description of his plate is most accurate, for it exactly resembles the large volcano represented in charts of the moon. As a specimen of the author's mode of expressing himself we quote the following passage, with which he concludes one of his cases:—"No *post mortem* examination was made; thus ended the unfortunate case, which might have been attended with much more comfort to the patient had his constitution been better, and the laryngeal consumption less extensive than it was."

Chapter II.—"Diseases of the Vocal Cords, of a Functional and Organic Nature, giving rise to Hoarseness and Modifications of the Voice, with Loss of it." In this chapter we have again to find fault with Dr. Gibb for not giving particulars of treatment. In one case he says, he "applied showers of strychnine to the cords." Imagine a physician, or one of Dr. Gibb's surgeons, "who cannot devote that patient attention and care" to "investigation and treatment," hastening to Dr. Gibb's book for information as to how he should treat a case of functional aphonia, having heard that "no effort had been spared to make it a useful and practical work—one that could be consulted on emergencies." He turns to page 109 and finds that Dr. Gibb, in a case of functional aphonia:—On the 16th applied showers of strychnine. On the 18th a solution of nitrate of silver and mercury, and a counter-irritant to the pomum. On 19th a solution of nitrate of silver in showers; repeated 20th and 21st. What an excellent guide to the poor physician or ignorant surgeon whose art "does not permit of care and attention in investigating disease." Nor will he be much more assisted by the reading of the following case in the same chapter:—

"CASE.—Aphonia for six months, wholly functional, cured by

metallic showers." . . . "Speaks in a low, almost inaudible, whisper. The larynx was not diseased, but the vocal chords were seen motionless. For a few days she had metallic showers to the larynx, of silver, copper, and zinc, which strengthened the whisper," &c.

The next subject is organic aphonia. Under this head are placed inflammation of the chords and larynx, growths and tumours, and disease of the brain, hoarseness and its causes; and the chapter ends with "Pathological Modifications of the Voice." In this section we find the necessity for the glossary. Strange and fearful words meet our eye Contendophonia—we turn to the glossary and find—"A straining of the voice in declamation and oratory; a term proposed by the author." Acantophonia, and several others, for which we must refer our readers to the author's glossary.

We have passed over the section on growths and tumours, having, on a former occasion, given a very full notice of this part of the subject, with woodcuts.

The rest of the book is written very much in the same style; introducing every circumstance that might possibly induce the lay reader to consult Dr. Gibb, and giving very little to the practitioner who may take up Dr. Gibb's book as a guide to treatment of his own cases.

We cannot conclude without remarking one novelty, entitled:—"Saccharine Throat, a New and Distinct Malady." This is the climax of mysticism in Dr. Gibb's book, and is quite beyond our experience; but, as Dr. Gibb gives us very little information about it, our readers must be content with the mere mention of what we are at a loss to describe.

While we condemn the style of the book, and the unsatisfactory way in which all the subjects are dealt with in it, we give Dr. Gibb our unqualified approval for the ardent zeal with which he has prosecuted his studies in laryngoscopy. We hope that in another edition of his work he may glean out the valuable information he has diluted with so much unnecessary verbiage, and give to the profession a more detailed account of the treatment with which he has been so successful. The book ends with a description of the laryngoscope, with which, in our opinion, it ought to have commenced.

The Influence of Weather upon Disease and Mortality, by R. E. SCORESBY-JACKSON, M.D., F.R.S.E., &c.

THIS work is reprinted from Part II. of Vol. XXIII. of the *Transactions of the Royal Society of Edinburgh*, before which body it was read by Dr. Scoresby-Jackson, who is lecturer on Materia Medica and Therapeutics at Surgeons' Hall, and author of *Medical Climatology*, the *Life of Dr. Scoresby*, &c. Beside that literary *sine qua non*, the "Introduction," the work consists of three parts (40 quarto pages); three tables, A, B, and C; and five illustrative plates.

Having referred to the name of Hippocrates, and other medical fathers, as commonly associated with the subject, the author thus writes:—

"The influence of weather upon disease and mortality has been acknowledged as a potent external force in every age, from that eminently speculative and credulous period when physicians professed to receive their diagnostic as well as their therapeutic inspirations from the stars down to our own day. And yet there is, perhaps, no question in the whole cycle of medical sciences which has made slower progress than the one we have now to consider. People believe that the weather affects them. They speak of its influence, sometimes commendingly, more frequently with censure, on the most trivial occasions; but beyond a few common-place ideas, the result of careless observation, or perhaps acquired only traditionally, they seldom seek a closer acquaintance with the subject. Our language teems with medico-meteorological apophthegms, but they are notoriously vague. The words which are most commonly employed to signify the state of the weather at any given time possess a value relative only to the sensations of the individual uttering them. The general and convertible terms—bitter, raw, cold, severe, bleak, inclement, or fine and bracing—convey no definite idea of the condition of the weather; nay, it is quite possible that we may hear these several expressions used by different persons with reference to the weather of one and the same place and point of time."

Having glanced at the paucity of medical inquiry into this subject, and referred to names of home and foreign writers on it, the author advocates the utility of such investigations, because whether we do or do not possess a knowledge of the etiological and therapeutic influence of meteorological phenomena, we invariably

act as if we were most intimate with the subject. Pursuing the same strain, he remarks that in estimating the value of a foreign climate, we are not to depend so much upon a comparison of the meteorological data of the several places, as upon the relations subsisting between the meteorological data and the prevalent diseases and death-rate of one and the same locality.

Scotland is selected as the subject of investigation; the meteorological data are taken from the collected returns from the stations of the Meteorological Society of Scotland, as reduced by the Astronomer Royal. The amount of rain in inches, the number of rainy days, and the degree of saturation, are given as deduced by Mr. Glaisher; while the mortality tables are constructed from the returns made by the Registrar-General. The red dots on the map of Scotland, facing the title page, indicate the situations of the meteorological stations; the red lines, the positions of eight large towns—Glasgow, Edinburgh, Aberdeen, Dundee, Perth, Greenock, Paisley, and Leith. The diagrams, coloured and clearly drawn, are of considerable value, and serve to elucidate the larger tables. Table A represents a gradually descending ratio of mortality; B represents the meteorology, and the death-rate from all and several causes at all ages, and from all causes at several ages, in the consecutive order of the months in each year; table C represents a comparison of the meteorology and mortality of the several corresponding months of the different years.

Part I. consists of ten sub-divisions, which we shall attempt to analyze:—

I.—THE INFLUENCE OF WEATHER UPON MORTALITY FROM ALL CAUSES.

Here the author draws attention to the difference between mortality and disease: "we shall fall into error if we suppose that the season of highest mortality is always the season of greatest sickness. It not unfrequently happens that certain seasons which are characterized by a maximum of sickness, are, at the same time, distinguished for their low rate of mortality; and contrariwise, seasons which may be somewhat remarkable for the general health of the public, may, by their influence upon one or two classes, present a high death-rate. Having abandoned the *morbility* statistics "as next to worthless," he proceeds to point to disease through mortality only.

Using the terms *maximum*, *major*, *minor*, and *minimum* of

mortality, the following table of the mortality of six years shows the distribution of the months:—

	Maximum.	Major.	Minor.	Minimum.
January,	4	1	1	–
February,	3	3	–	–
March,	4	1	1	–
April,	2	2	2	–
May,	–	3	2	1
June,	–	–	4	2
July,	–	1	2	3
August,	–	1	–	5
September,	–	–	1	5
October,	–	–	4	2
November,	1	5	–	–
December,	4	1	1	–

The order of the months, as determined by an average of the six years, is as follows:—

		Death-rate.			Death-rate
Maximum,	January,	265·3	Minor,	May,	219·5
	February,	257·4		June,	208·6
	March,	249·8		July,	204·5
Major,	December,	247·9	Minimum,	October,	198·2
	April,	242·8		August,	189·4
	November,	237·1		September,	187·7

Mean, 225·7.

The six years are 1861, '62, '60, '57, '59, and '58, and the relations which the months of the different years bear to each other are thus set down:—

	Maximum Mortality.	Major.	Minor.	
1861,	304·1	—	—	Mean, 265·3.
1862,	296·6	—	—	
1860,	280·8	—	—	
1857,	253·2	—	—	
1859,	—	243·4	—	
1858,	—	—	214·0	

The four Januaries in the *maximum* column are those of 1861, '62, '60, and '57 ; the January in the *major* column is that of 1859; and that in the *minor* is of the year 1858.

Taking first the maximum section, we find that January, 1861, was intensely cold, with unusually high barometrical pressure. January, 1862, was remarkable for its great depth of rain, large number of rainy days, and small amount of sunshine, with a high mean temperature. January, 1860, had a mean temperature less than average of preceding Januaries, with less sunshine, greater amount of cloud, greater amount of humidity, and rain as well as unusual preponderance of north-east wind. This was the most severe January of any during, at least, thirty-four preceding years. So much for the weather of those months whose death-rate is above the average of the six years.

January, 1857, weather average, began and ended with a snow-storm. This concludes the analysis of the maximum section.

January, 1859.—Weather mild, but unusually stormy and rainy: winds, W. and S.W. January, 1858.—Very mild. "At Sandwick, wall-flower, stock, carnation, and borage were in flower, so as to yield a bouquet on the 1st of January, while the hepaticas were in flower on the 4th. At Aberdeen, the hazel and snow-drop were in flower on the 25th; and at Banchory House the *rhododendron ponticum* was in flower on the 29th; the thrush was often heard singing during the month at Scourie, and the lark at Aberdeen."

From these data the author concludes "that the general term of a 'mild' or 'open' January corresponds with a low rate of mortality, a 'severe' January with a high rate of mortality." On a similar plan, Dr. Jackson considers the months of March, December, November, and August. Of these we need only remark on the anomaly of November, 1858, which, with the lowest death-rate of all the Novembers, "was cold, wet, and windy to an unprecedented degree. The barometer was lower, and more uniformly low, than in any month of the last six years. The mean temperature was lower than in any November through the same time." August and September, 1857, present an exceptional death-rate, particularly the former month, of which the Registrar-General reports:—"The mean temperature in August realized the very unusual height of 60°; and as July, also, and the beginning of September, had mean temperatures higher than usual, bowel complaints (diarrhea, dysentery, and cholera) became so prevalent and fatal, that instead of only fifty-six dying from these complaints in every hundred thousand persons, as in 1856, no fewer than 112 deaths occurred, in a like population, in 1857."

The Influence of Temperature upon Deaths from all Causes.—The

relationship existing between temperature and deaths, from all causes, is described as follows:—

Mean of	Mortality.	Mean Temperature.
Maximum Section,	269·49	38·3
Major ,,	233·25	43·1
Minor ,,	214·26	49·5
Minimum ,,	185·85	54·3
Mean of the 72 months,	225·71	46·3

From another table, the figures of which could not well be set before the reader in the present brief notice, the conclusion is arrived at, " that for every diminution of mean temperature below 50°, there is a corresponding increase of mortality; but that from mean temperatures above 50° a diminution is favourable to vitality, at least if the temperature have been for any length of time above 50°. In other words, mean temperature and mortality from all causes have an inverse relationship below 50°, a direct relationship above 50°."

Monthly Range of Temperature.—The relation of monthly range of temperature to the death-rate from all causes is shown in Table A, the means of the four sections being as follows:—

Section.	Mortality.	Monthly Range of Temperature.
Maximum,	269·49	36·4
Major,	233·25	37·2
Minor,	214·26	39·6
Minimum,	185·85	33·9

From this and another table, it is inferred " that for three quarters of the year the relationship of monthly range of temperature and the death-rate from all causes is inverse, the greater the range the lower the mortality; but that during the months of July, August, and September, the relationship is direct—the greater the range the greater the mortality."

Mean Daily Range of Temperature.—From two tables a similar relationship is indicated between mortality from all causes and the mean daily range of temperature, as was noticed with respect to the monthly range of temperature.

The Combined Influence of Temperature and Humidity.—The relative effect of a dry and a humid cold upon the death-rate is

shown in the following table, which we give as a specimen of many others:—

Months.	Mean of the Six Years				Dry Cold				Humid Cold			
	Mean Temp.	Rain-fall	Humidity	Mortality	Mean Temp.	Rain-fall	Humidity	Mortality	Mean Temp.	Rain-fall	Humidity	Mortality
	°				°				°			
January, - -	37·4	3·82	88	265·3	35·7	2·77	87	253·2	35·5	4·56	89	280·8
					39·3	2·98	86	214·0	39·6	4·21	87	243·4
February, -	38·2	2·32	87	257·4	39·3	1·54	89	250·1	39·5	3·32	88	228·1
					40·1	1·88	89	251·3	40·5	3·38	88	246·9
March, - -	39·8	3·55	86	249·8	38·4	3.52	86	283·2	37·8	3·63	88	256·7
					39·5	1.95	85	257·8	39·2	2·94	86	250·0
April, - -	43·2	2·11	82	242·8	41·5	1·18	82	290·2	41·3	3·20	80	222·7
					43·8	1·86	78	227·1	44·6	2·99	83	250·0
Means, - -	39·7	2·95	86	253·8	39·7	2·21	852	253·4	39·7	3·53	86·1	247·3

In this table there is a tendency to support the general belief that a *dry* cold is more fatal than a *humid* cold. As opposed to this view, however, may be taken the data of the Januaries and Aprils; and the reader's attention is directed to a table on p. 24, where is exhibited the growing mortality consequent upon a continued low temperature; as also to another, showing the evil effects of continued high temperature.

The Influence of Vicissitudes of Atmospheric Pressure.—The reader is here reminded "that the average monthly range of the barometer for the six years under examination is not more than 1·262 inches," and, consequently, that much importance must be attached to its oscillation, even to the extent of a single line. Under the head *Mean Height of Barometer Reduced to Sea-level and* 32 *F*, we find several tables of more or less intricacy; the general conclusion to be deduced from them may be stated thus:—That high and low barometric readings are both more conducive to vitality than a medium reading; and that a low barometer is more fatal than a high one. Further, it would seem "that over the whole year, and in the seasons of Winter, Spring, and Summer, the relationship between the mean monthly height of the barometer and the death-rate is *inverse*, but that in the Autumn it is *direct*." From two tables the author arrives at the conclusion "that the mortality from all causes bears a direct relation to the range of the barometer. The greater the range of the barometer the greater the death-rate, and *vice versa*."

The Influence of Drought and Humidity.—The influence of the relative amount of moisture upon mortality is tested from three

points. *First*, with reference to the number of rainy days; *second*, with reference to the quantity of rain-fall in inches; and, *third*, with respect to the relative saturation of the atmosphere with moisture, as ascertained by means of Mr. Glaisher's hygrometric tables. "A rainy day" being here considered as a day on which any amount of rain falls, the general indication, from various tables, is—that the mortality is greatest in those months which have the greatest number of rainy days; and that the relationship existing between the amount of rain-fall and the mortality from all causes is direct over the whole year. Under the head *Humidity*, some data in one table would indicate that dry air, irrespective of temperature, is more fatal than a humid atmosphere; while others tend to corroborate the opinion that *dry cold* is more fatal than humid cold.

On the Influence of Certain Winds.—From the first table under this head we have a general indication of a direct relationship between mortality from all causes and winds from a point between S. and W. The due E. wind shows little or no predominance in one section more than another. The S.E. wind shows a tendency to blow directly as the mortality, but not uniformly through all the four sections. The due S. wind, with the exception of the first section, blows inversely as the mortality. The N.W. wind affords no determinate relationship. From a second table it may be inferred that all winds between N.W. and S.E. (North about) are directly related to the death-rate; whilst those winds blowing from points between S.E. and W. (South about) have an inverse relationship. It would seem, moreover, that calms, or slight shifting winds, are less frequent when the mortality is high than when it is low.

Under the head *Force of Winds* the pressure of the wind is given in lbs., and the deduction is, that whatever be the exact pressure of the wind, it acts in a twofold manner on the death-rate, each influence compensating the other. Thus, by high winds and stormy weather, the death-rate will be increased by casualties afloat and ashore, and the fatal exhaustion of debilitated persons. On the other hand, a low pressure of wind causes atmospheric stagnation, and loads the air with pestilent effluvia, beside promoting the spread of zymotic diseases. A breeze of wind on land sweeps out the hot-beds of disease, clearing them of the products of animal and vegetable putrefaction, and rendering them wholesome habitations for the human family: just as in another sphere of usefulness it turns up the surface of the wide ocean, preserving its waters from corruption, and imparting the very essence of life to its creatures.

Repudiating the notion that his treatise exhausts the subject of it, or even the information to be derived from the tables, the author claims nothing more than his having examined its leading characteristics, without endeavouring to propound or substantiate any theory whatever. He then recapitulates the inferences or indications, which occupy only one page, and proceeds to consider—

II.—THE INFLUENCE OF WEATHER UPON MORTALITY FROM SPECIAL CAUSES.

The first head considered under this second division of the subject is:—A. *The Influence of Weather upon Mortality from Zymotic Diseases.*—The zymotics are here treated as a class; but in the tables we have the death-rates from three specific diseases—namely, typhus, scarlatina, and diarrhea. It would seem that during the colder months we have an *inverse* relationship between the height of the barometer and the death-rate from zymotic diseases, while during the warmer months the relationship is direct. One of the tables tends to prove that with a high death-rate from zymotics, there was also *breezy* weather, contrary to what might fairly have been presumed. No explanation can be given as to the share of the weather in influencing fluctuations in the death-rate from zymotic diseases, though no one will doubt that such influence really exists. B. *The Influence of Weather upon Mortality from Phthisis Pulmonalis.*—Taking phthisis as the fair and most fatal type of tubercular diseases, we find, from three tables:—

"1.—That a low mean temperature of the Winter months gives rise to an increase in the death-rate from phthisis, and that this relationship is the more clearly observable if the low temperature be sustained for some time without intermission, as in the case of the months from November, 1859, to February, 1860, inclusive. A high Summer temperature does not seem to increase the fatality of phthisis. It is only when the temperature of Winter is remarkably low that the increased death-rate from phthisis is distinctly traceable to that cause.

"2.—That the relationship between the monthly range of temperature and the death-rate from phthisis is uncertain, and that the latter is not under the control of the former.

"3.—That the daily range of temperature exerts no constant influence upon the death-rate from phthisis.

"4.—That there is no constant relationship observable between the mean monthly height of the barometer and the death-rate from phthisis.

"5 .—That if there be any indication of a constant relationship between the monthly range of the barometer and the death-rate from phthisis, it is that the death-rate increases with the range.

"6.—That the rain-fall bears no constant relationship to the death-rate from phthisis. It is possible, however, that it may be inverse in the colder, and direct in the warmer months.

"7.—That possibly an increase in the number of days, during which north, north-east, and east winds prevail, may give rise to an increase in the death-rate from phthisis."

C. *The Influence of Weather upon Mortality from Bronchitis.*—After some very sensible observations on the grounds of a choice of climate in diseases of the respiratory organs as compared with phthisis, the author elaborates three tables, from which, if they are to be trusted, it would seem:—

"1.—That there is an inverse relationship between temperature and the death-rate from bronchitis in all seasons, but that this is more remarkable in the Winter months, and especially when the cold is severe and protracted.

"2.—That possibly there may be an inverse relationship between the monthly range of temperature and the death-rate from bronchitis over the whole year, but the relationship varies with the season.

"3.—That the relationship between the daily range of temperature and the death-rate from bronchitis also varies with the season, but there is no indication of any constant correspondence.

"4.—That possibly the relationship between the mean height of the barometer and the death-rate from bronchitis may be inverse in Summer, and direct during the remainder of the year. And that there is no constant relationship between the death-rate from bronchitis and the monthly range of the barometer.

"5.—That the rain-fall does not influence the death-rate from bronchitis, . . . but that the state of the barometer, and the hygrometric condition of the atmosphere, do exert a powerful influence upon the mortality from bronchitis, and that the reason why such influence is not more distinctly visible is this—that whilst a dry atmosphere with a high barometer is prejudicial to one class of bronchitic patients, it favours another, and *vice versa*—so that the one class balancing the other, the influence is not discoverable upon the whole death-rate.

"6.—That the north, north-east, and east winds decidedly tend to increase the death-rate from bronchitis."

D. *The Influence of Weather upon Mortality from all Causes, at*

Different Ages.—Under this head we have the following deductions:—

"1.—That a protracted low temperature in Winter largely increases the death-rate amongst children under five years of age; and that the death-rate rises almost immediately upon the fall of the thermometer, and falls again so soon as the temperature begins to rise.

"2.—That a continued low temperature perceptibly increases the death-rate amongst those between five and twenty years of age, though to a much less extent than in infancy; and the mortality curve does not rise so suddenly upon the fall in the curve of temperature.

"3.—That continued cold also raises the death-rate amongst adults, more perceptibly than in youth, but less than in infancy.

"4.—That in old age continued cold is prejudicial, but the death-rate does not rise so suddenly as either in infancy or in adult life.

"5.—It would appear from the foregoing remarks, that severe Winter weather induces acute inflammatory diseases in infancy and adult life, rapidly cutting off its victims; that it increases the death-rate of the aged by aggravating chronic diseases; and that it probably cuts off only those in youth who are previously debilitated by some exhausting disease, as phthisis.

"6.—That a high temperature in Summer, especially if long sustained, increases infantile mortality.

"7.—That such high Summer temperature scarcely affects the death-rate in youth.

"8.—That it slightly increases the mortality in adult life.

"9.—And that it also slightly increases the death-rate of the aged.

"10.—That care ought to be taken to avoid exposure to the direct influence of the weather when the mean temperature sinks below 39° in Winter, or rises above 57° in Summer."

A "general *resumé*," and a number of elaborate tables, bring this work to a conclusion. There can be no question of the fact, that treatises, such as this, tend greatly to raise our profession in the eyes of the educated laity, and to place it in its true position before scientific men. We beg to congratulate Dr. Scoresby-Jackson on the publication of his treatise, and we cordially recommend it to the thoughtful study of our profession.

Lectures on Syphilitic and Vaccino-Syphilitic Inoculations; their Prevention, Diagnosis, and Treatment. Illustrated by Coloured Plates. By Henry Lee, F.R.C.S.; Senior Surgeon to the Lock Hospital and Asylum, &c., &c., &c. Second Edition. London: Churchill and Sons. 1863. 8vo, pp. 335.

Mr. Henry Lee's views as to the distinctions between the different modes of origin of different syphilitic affections were, for the first time, pointed out in 1854, when the first edition of the lectures, now before us, was published. Since then these distinctions have attracted a large share of public attention at the hands of both English and Continental writers, and assuredly, well deserve the careful consideration of observers who are desirous of elucidating the still profoundly perplexing phenomena of syphilitic affections.

Various authors who have written during the present century on the subject of syphilis may be thus classified according to the doctrines which they have respectively advocated:—

1st. Those who, with John Hunter, regarded all syphilitic affections, including gonorrhea, as depending upon the same poison.

2nd. Those who distinguish between gonorrhea and syphilis, and ascribe them to the action of poisons essentially different in their nature.

3rd. Those who believe in the plurality of venereal poisons, conceiving that several distinct forms of primary venereal sore exist, each liable to be followed by a train of secondary symptoms peculiar to itself.

4th. The school, of which Ricord is the champion, which admits the difference between gonorrhea and syphilis, but divides the latter into two, viz., syphilis which infects the patients constitution and that which does not.

Some authors of the last class regard each of these diseases as arising from a separate poison, and consequently acknowledge three poisons as habitually producing contagious diseases of the generative organs, that is to say, three distinct venereal diseases, of which one only is liable to be followed by the phenomena of secondary venereal contamination.

To the last group Mr. Henry Lee virtually belongs, without, however, admitting that the existence of each poison is capable of demonstration. "It does not follow," he observes, "that

because we can distinctly trace so many morbid processes, that there are the like number of morbid poisons. The diseased actions we can trace with great precision, and can predict the circumstances under which they will occur, and define distinctly the laws by which they are regulated and controlled. It is, therefore, more in accordance with strict inductive science, simply to describe the different *modes of action* which occur after impure contact, than to ascribe each different action to a different poison."

In these lectures, then, the author treats, for the most part, of the morbid processes to which the contact of syphilitic matter gives rise, and of the results which follow. The kind of chancre, commonly known at present as the non-infecting, he designates as the *primary suppurating syphilitic sore;* he describes it as a local disease, never producing secondary symptoms, commencing as a pustule, and running a definite course, not beneficially influenced by mercurial treatment, though sometimes tedious (three or four months) in healing.

"When artificially inoculated, the inoculated point becomes red within the first twenty-four hours. From the second to the third day it becomes slightly raised, and is surrounded by a red areola. Between the third and the fourth day it contains a fluid more or less turbid. From the fourth to the fifth day the pustule becomes fully formed, and from this time to the termination of the disease the secretion consists of well-formed pus. Sooner or later, the cuticle covering the pustule is detached, and in some instances it may be removed at the time of the inoculation, whether artificial or natural. This alters the appearance of the affection, but in nowise interferes with its essential characters. As soon as suppuration commences, there is a loss of substance in the part, and an ulcer forms, which has peculiar characters. When not interfered with by any accidental causes, it increases equally in every direction, so as to form a more or less perfect circle. The edges of the ulcer are cleanly cut, and present a sharp outline. The appearance presented is often that of a piece of skin having been removed by a punch. The edges of the ulcer are frequently slightly undermined and everted. The surface of the ulcer is irregular, sometimes presenting granulations, at other times presenting the appearance of having been worm-eaten. Often the bottom of the ulcer is covered by an adherent, greyish, tough matter, which probably is a part of the natural texture which has undergone a kind of molecular necrosis, and is in process of being separated from the subjacent living parts. . . . The suppurating syphilitic sore gradually increases during a certain period, then remains stationary, and finally heals."

Various accidental causes may modify the appearance and progress of this typical form of suppurating chancre:—

1st. If destroyed by caustic within the first five days, a simple ulcer alone remains.

2nd. Its shape and appearance may be modified by meeting, during its progress, with different textures.

3rd. If the specific inflammation extends to the areolar tissue, inflammatory exudation takes place there, causing an induration closely resembling that which accompanies the infecting chancre, and gives rise to the *phlegmonoid variety* of suppurating syphilitic sore.

4th. Lymphatic absorption of the product of the sore giving rise to a bubo secreting specific pus.

According to Mr. Henry Lee, the local symptoms which are the forerunners of syphilitic infection of the system are extremely difficult to recognize; syphilitic infection does not manifest itself immediately on the application of the poison; there is a period of incubation lasting from three to seven weeks, after which the primary disease will manifest itself, commencing as a crack, an abrasion, or a pimple. When the disease has thus declared itself, cauterization is of no avail to prevent constitutional symptoms. When the period of incubation is once passed the appearance of a sore is in itself proof that the system is already infected. "Infecting sores that have been destroyed on the very day of their appearance have subsequently continued to spread, and have produced their natural consequence. Even if the sore be cut out the infection of the patient's system will not thereby be prevented." When this terrible enemy which has been lying in ambush during the weeks of incubation manifests itself, it does so under one of these forms, all of which are modifications of the adhesive kind of action.

1st. The cuticle may appear as if peeled off from the glans; or a circumscribed patch, of a livid or purple colour, may remain for days, and continue without any specific induration, merely throwing off epithelial scales and lymph globules.

2nd. An indurated tubercle may form in the skin, or under the mucous membrane, with the specific induration well marked, but without ulceration.

3rd. The most usual form, known as the indurated or Hunterian chancre, in which the specific induration co-exists with ulceration.

The first two forms in which syphilitic infection declares itself

have not, as Mr. Lee conceives, been recognized by the majority of writers on syphilis, and hence many cases have been erroneously recorded in which constitutional syphilis was supposed to have been acquired without any primary affection. The characteristic induration accompanying the second and third form is the diagnostic test of the true nature of the complaint; it terminates quite abruptly, and is as if a piece of cartilage or a pea were introduced under the skin or mucous membrane. Yet this induration, however valuable as a proof of the infecting character of the sore which it accompanies, cannot always be distinguished from the hardness which surrounds the phlegmonoid variety of suppurating (non-infecting) sore. The diagnosis, nevertheless, of the one from the other is of much importance, for admittedly the latter form of chancre is "not beneficially influenced by mercurial treatment;" and the practitioner who unnecessarily mercurializes a patient is inexcusable. What, then, it may be asked, are the diagnostic points on which Mr. Lee relies in this case? On the microscopic appearances of the secretions furnished by the sores (as figured in the volume before us), and on the inferences to be drawn from the re-inoculability, on the patient himself, of these secretions. This latter is indeed a matter of the greatest interest as regards the natural history of venereal disease.

As our readers are very well aware, John Hunter and his followers believed that the inoculation of gonorrheal matter was followed by the production of a syphilitic sore; in other words, that one and the same virus gave rise to gonorrhea and syphilis. One of the first authors who successfully combated this view was J. F. Hernandez,[a] who, in a prize essay presented, in 1810, to the Medical Society of Besançon, came to the conclusion of the non-identity of gonorrhea and chancre. His observations were made on criminals. Having made experiments, by inoculation, on seventeen individuals, and closely watched the result, he concludes:—"My experiments prove that the ulcers which are produced by the inoculation of the gonorrheal virus are not syphilitic." It was not, however, until, in 1838, when Ricord published his *Traité Pratique sur les Maladies Vénériennes*, that the true value of inoculation began to be generally recognized. At this time, which must ever be remembered as an important epoch in the history of syphilis, Ricord daily demonstrated to his pupils the facts—

[a] Essai Analytique sur la Non-identité des Virus Gonorrhoïque et Syphilitique. Par J. F. Hernandez. Toulon, 1812.

1st. That when pus, taken from the urethra of a patient suffering from gonorrhea, was inoculated upon the patient, no result followed.

2nd. That when pus, taken from a chancre (a non-infecting or primary suppurating chancre) was inoculated with a lancet on the same patient, a chancre (of the same kind) was always produced.

When Ricord established, beyond dispute, this important point as to the difference between gonorrhea and syphilis he had not yet recognized the difference, as regards inoculability, between the simple, or *non-infecting* (suppurating chancre of Lee), and the infecting chancre. No one can doubt that much of the more precise knowledge which has, of late years, been gained, with reference to the inoculability of the virus of the infecting chancre, is due to the keen observation of Mr. Henry Lee. Few syphilographers deny the truth of Ricord's assertion of 1838, if it be admitted to apply to the simple non-infecting chancre. This kind of chancre may be repeatedly inoculated on the same individual, producing a sore like the parent. Ricord says its inoculability is its only certain diagnostic mark. One very important and interesting fact, concerning the re-inoculation of this kind of chancre, requires especial notice, viz.:—

"That after repeated inoculations have been made on a part, that part becomes less and less susceptible to the influence of the poison, and a time arrives at which the inoculations will cease to secrete pus, and then they will no longer be inoculable. If fresh matter, however, be used, the inoculations will again succeed; but these will gradually lose their effect, as at first. This process may be repeated until a part is no longer susceptible to any inoculation from the secretion of a suppurating syphilitic sore. But then a fresh part may be inoculated, and the same process repeated. Under this mode of treatment it is said that a time ultimately arrives at which no further inoculation can be effected from a suppurating sore upon any part of the body. Even then, however, after the lapse of a certain interval, the suppurating syphilitic sore may again be communicated, but always without imparting any constitutional or syphilitic taint to the patient."

According to Ricord's later views an indurated chancre is never produced more than once in the same subject. If this dogma be accepted, one is prepared to admit the fact, which M. Clerc announced in 1855, and which Mr. Henry Lee tested experimentally in the Lock Hospital, London, in 1856, viz., that once the characteristic induration has been established an infecting chancre

can no longer be re-inoculated on the same individual. Inoculation with the virus of an indurated chancre, performed upon the individual who has the chancre, is either sterile or produces a sore without induration, rarely ulcerating; and, if so, healing up soon. There is, upon the whole, considerable unanimity among the syphilographers of the last eight or ten years upon this subject; and some very remarkable statistics have been brought forward to show that the virus from an infecting sore cannot be re-inoculated upon a person who has already had an infecting sore, so as again to produce an indurated chancre and a second time to contaminate the system. In fact, inoculation with the virus of an indurated sore, performed upon a person who has already had such a chancre, is either sterile or produces a sore without hardness, but which propagates itself in a third healthy person as an indurated (infecting) sore.

If the virus of an infecting chancre be taken during the early stage, before the chancre has become indurated, then it may be inoculated upon the same individual so as to produce an indurated sore. The induration is the first symptom of a diathesis; it is not only the necessary prelude to constitutional syphilis, but it is, in itself, evidence that the system is already contaminated. Hence, after it has once taken place, no other indurated chancre can be again produced in that individual. Once a sore has declared itself, by its characteristic induration, to be an infecting chancre, cauterization is of no avail. "A sustained, judicious, and constitutional mode of treatment is the only one that can be relied upon in the treatment of this disease." As to the advisability of a steady, prolonged mercurial treatment, for the cure of true syphilis, Ricord and Mr. Henry Lee may be said to be in harmony.

Mr. Henry Lee, as appears from the foregoing imperfect analysis of his views, belongs to the class already mentioned which admits the difference between gonorrhea and syphilis, and regards the latter as consisting of the infecting and non-infecting forms; whether these forms of disease be regarded as arising in all cases from essentially distinct poisons, or whether they may be regarded as the result of "*the different modes of action*" of one and the same poison makes little practical difference. According to Mr. Henry Lee and other learned syphilographers, the one contaminates the system, the other never does; the one requires mercury for its cure, the other does not. In establishing, therefore, with considerable precision, the peculiar characteristics on which is based the differential diagnosis between these two forms of chancre, the

writers on syphilis of later times have done a great service to mankind; they have driven mercury back to its last stronghold, and have limited the mercurial treatment of venereal diseases to true infecting syphilis. How terribly has mankind suffered while scientific observers were slowly advancing to this point of knowledge. It is, indeed, interesting, and at the same time, we must confess, humiliating, to look back over the history of the mercurial treatment of venereal diseases. John Hunter, regarding all venereal diseases as arising from a common virus, recommended mercury, even in the treatment of gonorrhea. In speaking "of the treatment of the constitution in the cure of gonorrhea,"[a] he says, "whatever methods are used for the cure, either locally or constitutionally, it is always necessary to have in view the possibility of some of the matter being absorbed and afterwards appearing in the form of a lues venerea, to prevent which I should be inclined to give small doses of mercury internally." Gently as this recommendation was given, we know to what a frightful extent it was followed by the pupils of the great man who gave it. Many among us may still remember the bold, vigorous, and fearless language in which Sir Astley Cooper, not very long before his death, in 1841, denounced gonorrheal mercurialists, who, blinded by prejudice, still pursued their horrid practice, no doubt, in perfect sincerity. "It is lamentable to think," says Sir Astley, in his lectures,[b] "on the number of lives which must have been destroyed by phthisis and otherwise, in consequence of the imprudent exhibition of mercury which prevailed among the older surgeons for a disease which did not require it. At the present time, however, a surgeon must be either grossly ignorant or shamefully negligent of the duty which he owes to the character of his profession, and to the common dictates of humanity, if he persists in giving mercury for this disease. Let those persons who suppose that gonorrhœa can be cured by mercury, go round our wards, and see whether mercury has any effect on that disease. Look, gentlemen, at 100 patients in our foul wards, many of whom come into the hospital with syphilis and gonorrhœa; and many, I am sorry to say, who have only gonorrhœa, but who are invariably carried to these wards. What is the miserable treatment of these patients? You are aware, gentlemen, that I scarcely ever enter the foul wards of the

[a] A Treatise on Venereal Diseases. By John Hunter. London: 1786. P. 86.

[b] Lectures on the Principles and Practice of Surgery. By Sir Astley Cooper, Bart. Fourth Edition. London: 1835. P. 464.

other hospital;—when a particular case demands my attention, I have the patient removed to a clean ward. I will tell you why I do not enter those wards, gentlemen. I abstain from entering them, because patients under gonorrhœa are compelled to undergo so infamous a system of treatment that I cannot bear to witness it. To compel an unfortunate patient to undergo a course of mercury for a disease which does not require it, is a proceeding which reflects disgrace and dishonour on the character of a medical institution. No consideration shall induce me to repress my feelings on this subject; no authority shall restrain me from giving full expression to those feelings. As long as I continue a surgeon of Guy's Hospital I will endeavour to do my duty; but I care not whether I continue a surgeon of that hospital another day. I do say that the present treatment of patients under gonorrhœa in these hospitals, by putting them unnecessarily under a course of mercury for five or six weeks, is infamous and disgraceful. The health of a patient is, perhaps, irremediably destroyed by this treatment; and, after all, not the slightest effect is produced by it on the disease. If he is cured of his gonorrhœa at all, he must be cured by other means. If you go to a patient for gonorrhœa in the foul wards, at the end of his course, and ask him how many times he has rubbed in, he will generally answer 'twenty-eight times.' If you ask whether he is salivated, he will tell you that he spits three pints a-day; but ask him whether his gonorrhœa is cured, and he will reply, 'No, I have a clap still upon me.' His disease is not in the slightest degree affected by the mercurial course to which he has been so unpardonably subjected, and it will soon after be necessary to cure him by injections or other means. When so infamous a practice prevails, I cannot satisfy my own feelings by resorting to milk-and-water language; every man of common feeling and honesty is bound to speak out on such an occasion."

Sir Astley Cooper, although admitting "that chancres, for which no mercury has been given, are not always followed by secondary symptoms," and well aware of its injurious influence on the constitution, nevertheless, advised the administration of mercury for the cure of chancres.

The keenly-observant Colles, doubted if any chancre would get well without mercury, and strongly advocated the mercurial treatment of venereal ulcers. "I once asked Mr. Pearson," he says, in his lectures, "when he was in full practice in these cases, if he

ever knew a chancre to heal of itself. His answer was, '*I think* I did.' So you may conceive how unusual such an occurrence is, if it takes place at all." Sir Benjamin Brodie, in his admirable observations on the administration of mercury in cases of syphilis, observes, "you are not then to suppose that you are to administer mercury, as a matter of course, in all cases of syphilis; but *the general rule is, that it should be given.*"

We are now taught, by Ricord, Lee, and other careful investigators, whose assertions are supported by many observations and experiments, that the kind of chancre which we most frequently meet with in practice (the simple, non-infecting, or suppurating) is a disease which does not contaminate the system, and which does not require mercury for its cure. To mercurialize in such cases is as unnecessary for the cure of this disease as it would be for the cure of gonorrhea; and the practitioners who, in the present state of knowledge, mercurialize patients for the cure of simple chancres, are deserving of the censure passed by Sir Astley Cooper on the gonorrheal mercurialists of his time. There cannot now be any reasonable doubt that much of the diversity of opinion, many of the strangely antagonistic views of writers on syphilis, as well as a deal of useless controversy in the debates which have taken place on the subject of venereal disease, must be attributed to the want of a clear understanding of the distinction between the different kinds of primary syphilitic sores. Those individuals who have striven to establish with precision this distinction, and who have endeavoured to lay down definitely the characteristics whereby the one form of disease may be diagnosed from the other, have done much to clear away the contradictions, inconsistencies, and confusion which surround the writings of syphilographers. They have done much to save mankind from the baneful influence of mercury uselessly administered. Among them the name of Henry Lee will not be forgotten.

We regret exceedingly that the limited space at our disposal does not permit of our giving an analysis of Mr. Lee's lectures on Syphilization (so called), Lymphatic Absorption, and Vaccino-Syphilitic Inoculation. We must content ourselves to draw to a close this notice of his truly valuable work, with a brief account of his doctrines regarding inoculation with the blood of syphilitic patients, and the transmission of secondary syphilis.

Mr. Henry Lee believes the fact to be incontestibly established that inoculation with the blood of a syphilitic person produces

syphilis in a healthy one. He grounds his belief, for the most part, on the experiments of Professor Pelizzari, who inoculated five persons with the blood taken from syphilitic subjects. In four out of the five the inoculation was sterile, and the result of the experiment negative. In one case (that of Dr. Bargioni, inoculated on the 6th of February, 1862,) a small papule made its appearance, at the seat of inoculation, about a month after the operation. This increased in size, so that on the eighth day it was as big as a twenty-centime piece. "On the eleventh day it was covered with a very thin adherent scale, resembling silver paper, which, upon the two succeeding days, became denser and less adherent, and in its central part commenced to crack. On the fourteenth day two axillary glands became enlarged to the size of nuts, and were movable and indolent. The papule remained indolent, but its sensibility was slightly increased. On the 19th pressure upon the crust caused a small amount of sero-purulent matter to exude from beneath its edges, the pressure giving a little pain. The axillary glands had now become larger and harder, but continued indolent. There was no induration apparent at the base of the papule. On the 21st the scale was transformed into a true crust, which had commenced to be detached at its edges; and the part beneath was ulcerating. Slight induration now appeared at the base. On the 22nd the crust was detached, and a funnel-shaped ulcer presented itself, with elastic and resistent borders, forming an annular induration. These edges were swollen, adherent, and obliquely inclined towards the base of the ulcer, which was covered with a very small amount of secretion. The pain was trifling. Dry charpie only was applied. On the 26th the ulcer had extended itself to the size of a fifty-centime piece. It secreted more, and the surrounding induration was considerably increased. Up to the 4th of April this ulcer remained stationary, but at that date its base appeared to be granulating. The corresponding glands remained swollen, hard, and indolent. There appeared at this date trifling nocturnal pains in the head, and the posterior cervical gland became somewhat enlarged. On April 12th there appeared upon the surface of the body, particularly upon the sides of the chest, and in the hypochondriac regions, spots of irregular form and of rose colour, unattended by any inconvenience to the patient. The glandular swellings of the neck were well marked. This eruption extended itself, and became more confluent during the succeeding days. No constitutional disturbance, heat of skin,

nor pruritus, accompanied this eruption, which went on increasing for eight days. On the 20th the cervical glands had increased in size, and were harder. The chancre maintained its specific character, and exhibited no tendency to cicatrization. On the 22nd the colour of the eruption was decidedly coppery. Small lenticular papules were now perceived to be mixed with the erythema. The edges of the chancre had begun to granulate. Mercury was now commenced."

It is necessary, as we think, to be cautious in concluding that the symptoms detailed in the foregoing experiment were really due to syphilitic contagion. "*Si in dubio suspice venerem*" is an adage that some are too prone to follow. It must be remembered that four other persons inoculated had no such results. Indeed when one reflects on the erroneous conclusions and the disastrous consequences which followed John Hunter's experiment,[a] made, we believe, on himself, by the inoculation of gonorrheal matter, we may well be unwilling to trust too much to the result of one experiment, on so intricate a subject. We are, however, on the whole, after a careful perusal of the cases and experiments collected by Mr. Lee, prepared to admit that there is a considerable amount of evidence to show that the blood of syphilitic patients and the secretions derived from secondary syphilitic affections are, under certain circumstances, inoculable upon persons who have not previously had the disease.

John Hunter, as our readers very well know, doubted the contagious character of the virus yielded by secondary syphilitic affections, although this fact had been generally admitted before his time. Had Hunter been aware of the fact that syphilis does not occur twice in the same subject, he would have viewed very differently the grounds which formed the basis for his deductions on this subject. Indeed, it is interesting to regard by the light of modern discovery some of his own recorded cases:—

"A young lady had a tooth transplanted, and the tooth fastened extremely well. It continued firm for about a *month*, when the gum began to ulcerate, leaving the tooth and socket bare. The ulcer continued, and blotches appeared upon the skin, and ulcers also in the throat. The disease was treated as venereal, and the symptoms disappeared, but they recurred several times after very severe courses of mercury. She at last got well.

[a] See Experiments made to Ascertain the Progress and Effects of the Venereal Poison.—Hunter on Venereal. 1786. Page 324.

"A gentleman had a tooth transplanted, and the tooth remained without giving the least disturbance for about a *month*. The edge of the gum then began to ulcerate, and the ulceration went on until the tooth dropped out. Some time afterwards, spots appeared almost everywhere on the skin. He was put under a course of mercury and all disease disappeared. Some time after, the same appearances returned, with the addition of swelling in the bones of the metacarpus. He was now put under another course of mercury more severe than the former, and in the usual time all the symptoms again disappeared. Several months after, the same eruptions came out again, but not in so great a degree as before, and without any other attendant symptoms. He a third time took mercury, but it was only ten grains of corrosive sublimate in the whole, and he got quite well. The time between his first taking mercury and his being cured was a space of three years."

Mr. Lee, after a very full, careful, and impartial consideration of the subject, concludes that "the experiments and cases which he records appear fully to establish the fact, that not only are the secretions from secondary symptoms inoculable under certain circumstances, but that the blood and the secretions of inflamed mucous surfaces in syphilitic patients (even when no distinct form of secondary disease is known to exist) may be the means of communicating the disease.

In conclusion, we beg to recommend to our readers Mr. Lee's work, in the strongest terms. It is a work of real merit, full of facts, well arranged, and viewed with care and originality.

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1. *A Practical Treatise on Diseases of the Skin in Children.* From the French of Caillault. Second edition, with Notes, Appendix, and Formulæ. By Robert Howarth Blake, M.R.C.S., Eng. Churchill. 1863.
2. *A Practical Treatise upon Eczema, including its Lichenous, Impetiginous, and Pruriginous Varieties.* By T. M'Call Anderson, M.D., &c. Churchill. 1863.
3. *Treatment of Diseases of the Skin.* By Dr. William Frazer, &c. Fannin. 1864.

We shall endeavour to group together what otherwise might be separate notices of the above books, deviating from this plan whenever the nature of the subject may seem to demand such

deviation. Caillault's work, already known to the profession, is here reproduced, for the second time, by Mr. Blake, with his own additions, as indicated on the title page; it consists of 331 pages, small octavo, and is clearly printed. Dr. Anderson's work has more pretensions to book-making than Caillault's; it is composed of 134 pages, large octavo; the type is very large, and the cases are spun out to the orthodox length. Dr. Frazer's book consists of 174 pages, foolscap octavo, and contains vastly more matter than Dr. Anderson's, while its type and binding are unexceptionable.

In Caillault's preface we find noticed what the author calls, in a child, "an apparent and a real age." Accordingly, throughout the work, much importance is attached to the study of skin diseases in relation to the evolution of the various ages. At the very commencement this idea is laid down, and with it another of equal importance—that skin diseases in children are, in most cases, symptoms of some constitutional affection. Diseases of infancy are arranged in nine groups, in the order in which they succeed each other as age advances, and not in their order of frequency.

The first section is on "Syphilis," and a considerable portion of the work is occupied with a discussion on phagedenic gangrene. Repudiating the notion that an infant may acquire syphilis by contact during the act of birth, M. Caillault dwells on what he considers a very common cause of acquired disease—infection from the breasts of nurses. He looks on a primary chancre during the first period of life as an exception, and therefore attaches most importance to the transmissible secondary affections, the "mucous patches," which exhibit all the characters of congenital disease. This he views as altogether cutaneous; and, remarking on the statement that it kills a large number of children, observes that, if it be true, "it is owing to the frightful rapidity with which they fall into the cachexia called syphilitic, which is so rapidly fatal."

The "mucous patch" M. Caillault considers the true pathognomic symptom of congenital syphilis; and not only so, but he further observes of this congenital affection that of it the mucous patch "constitutes the whole external manifestation."

As there is great diversity of opinion about this, we may tell our readers (what we are ourselves informed by Mr. Blake) that this "mucous patch" has been described under the following names:—Mucous tubercle, syphilitic tubercle, flat tubercle, flat pustule, syphilitic patch, humid papular syphilide, and condyloma. This "mucous patch," which has as many names as a prince, is neither

tubercular, pustular, nor papular; and we are warned that it should never be confounded with any of the tubercular syphilides, all of which are tertiary and non-contagious, whilst it is secondary and contagious. Mr. Blake, and, we are satisfied, a host of others, do not agree with M. Caillault, that the mucous patch is the sole indication of infantile syphilis. If we adopted any side in this question the controversy would only amount to a cannonade of authorities on both sides—just as Messrs. Peter Poundtext and Habbakkuk Mucklewrath battered each other with texts in the days of Old Mortality. The fact is, there is much to be said on both sides; and, having observed that M. Caillault has a good deal to say for his view, and that he discourses in a lengthened strain on the diagnosis of congenital syphilis, on onychia, vegetations, external appearance, roseola, erythema, and cachectic lesions, we come to the practical part—the treatment.

Commencing with the statement—which, doubtless, many of our readers will dispute or deny—that hereditary syphilis is rare, he recommends mercury, in the form of proto-iodide; and specially advises the treatment adopted by Professor Guillot. Beside the choice of a healthy nurse, or the use of good food, and other hygienic measures, this treatment consists in the use of a julep containing proto-iodide of mercury. The mercurial bath is recommended, and the strength laid down by Professor Guillot is ten grammes of corrosive sublimate with six grammes of hydro-chlorate of ammonia. M. Caillault says that salivation never results from the use of this bath. Mr. Blake does not think the use of strong mercurials at all necessary in infantile syphilis, and states, as the result of his own experience, that the most severe cases may be both speedily and safely cured by hydrargyrum cum cretâ and iodide of potassium, with a careful attention to the hygienic and dietetic means above advised. For our own part we think mercury, in any form, may often be dispensed with; and, when required, we think the mercurial garter of Sir Benjamin Brodie both the safest and most efficacious method of applying it.

Dr. Frazer's remarks on mercurials form Chapter I. of his therapeutical treatise. There is not much in it which we could notice in connexion with infantile syphilis, as its very moderate and fair observations are mainly intended for adult cases. However, in his appended "Formulary" may be found a good form for prescribing an iodide of mercury mixture, and also one for prescribing the chloride.

We need not pause while rapidly passing over the few pages of M. Caillault on "Strophulous Diseases." On their treatment he merely repeats the views of Rayer and Willan, taking a little from each, but adopting *in extenso* the opinions of neither. Dr. Frazer is more to the point. He recommends light and unirritating clothing, mild aperients and antacids, with a tepid bath, "to which some carbonate of soda, borax, or bran is a good addition."

A good deal of M. Caillault's dissertation on his third class, or "Lymphatic Diseases," is devoted to achor—the *tinea faciei* of Frank, the *porrigo larvalis* of Willan and Bateman, the *achore* of Alibert, and the *impetigo larvalis* of Biett. Distinguishing this eruption from eczema, he observes the perfect integrity of the mucous membrane of the eye, the nose, and the buccal cavity, while the disease renders every part of the face hideous to behold. Nor does he omit to notice, what certainly is an astonishing fact, that permanent baldness or cicatrices in the face never result from achores. The remaining species in this class are pernio (or chilblain) and molluscoid acne—*molluscum contagiosum* of Bateman.

In children the various forms of acne never present themselves, with the exception of that form just described; on the contrary, *acne simplex* is very frequent in adolescence, and *acne rosacea* is generally seen only in the adult. As to treatment of achores, M. Caillault recommends topical applications to moderate the painful itchings of the scalp and face; emollient lotions and cataplasms too long continued should be avoided. The lotions are frequently made with the nurse's milk obtained from the breast; and sulphurous lotions should be reserved for cases in which the achores have a tendency to be transformed into *dartrous* affections, such as eczema or lichen. Baths should be very short in duration, and rendered emollient with bran and gelatine, while the temperature should be very moderate. When a chilblain is marked by simple redness of skin, and a slight swelling of subcutaneous tissue, embrocations of oil of turpentine or of camphorated spirits of wine are recommended, as well as compresses soaked in Goulard's water. M. Caillault further says:—"The most reliable means are embrocations, with a decoction of oak bark, the skin having been previously rubbed with a spirit lotion." Tardieu has applied, with advantage, in cases of non-ulcerated chilblain, the following ointment:—Nitrate of silver, 1 gramme; lard, 30 grammes. Ratier has had satisfactory results from mercurial inunctions; and Jannyot uses a liquid as soon as the itching indicates the chilblain, but not afterwards; it is this:—Oak

bark, 50 grammes; alum, 30; red wine or water, 5,000. Reduce the decoction to two-thirds by boiling, and then add the alum.

The tumours of molluscoid acne, says Caillault, must be cut off or strangled by a ligature. Globular tumours should be opened, and the cavity cauterized; after which the most rational treatment consists in the use of tonics and other strengthening means.

Under the class "Scrofulous Diseases" we have short essays on cutaneous tubercles, subcutaneous tubercles, scrofulous ulcers, and lupus. Scrofulous ulcers in children are, unfortunately, so common in our workhouse hospitals, and the treatment of them, by good feeding, is so well known to all boards of guardians and ratepayers, that little need be said about them here. M. Caillault does not consider lupus as the type of cutaneous scrofula, though he includes it in the class. The force of his reasoning each man will estimate for himself, for it consists in asserting that "lupus is often found in vigorous plethoric subjects who have never presented any trace of scrofula." Three forms are described:—1, lupus affecting the surface; 2, lupus penetrating deeply; and 3, lupus with hypertrophy. This last kind he likens to, and his description amounts very nearly to that usually given of elephantiasis Grecorum. Beside the scrofulous diseases enumerated we also have impetigo.

Celsus has said, with as much truth of our day as of his own, *strumæ vel precipue medicos fatigare solent.* Accordingly we have here much elaboration touching fresh air, baths, exercise, country residence and "analeptic" diet; to say nothing of a perfect armoury of Materia Medica. M. Caillault recommends the acid nitrate of mercury as one of the best caustics for lupus, and speaks confidently of the efficacy of Dupuytren's powder, which the reader will find described among the arsenical formulæ in Dr. Frazer's book. In chronic impetigo Mr. Blake recommends the administration of small doses of sulphur in combination with glycerine; and places great reliance on the local application of liniments or ointments containing oil of cade or creosote.

The fifth class "Dartres (Tetters)," in M. Caillault's work, demands more than a passing notice. Beside the continental belief in the *dartrous constitution*, we must recollect that until the beginning of the present century the word *dartre* has, in France, been the synonyme of a chronic affection of the skin. Frank believed the dartres to be those cutaneous diseases which involved both the rete mucosum and the dermis, in contradistinction to porrigo, which only attacks the epidermis.

Dr. Anderson, in Chapter IV. of his book, writing of the preference which eczema exhibits for people of certain temperaments, declares that when it attacks the healthy and robust, in whom neither external nor internal irritation appears, it must be referred to some idiosyncrasy—"the *dartrous diathesis*, as the French call it, which is certainly a convenient word to cloak our own ignorance of its nature" This seems to be pretty much the same as the ominous nod, and the inscrutable word "constitution," which, in days not long gone by, threw a dignified shade over the ignorance of many a physician as he dismissed the disappointed patient from his study. The prurigo of children M. Caillault declares to be developed at once or successively over the whole body, and gives some directions for distinguishing it from eczema, scabies, and strophulous affections. Passing by his notice of lichen, which Alibert has described under the name of *humid squamous dartre*, we come to eczema. Having previously stated that there exists during lactation a disease called achor, M. Caillault says:—"At a later period, *i.e.* from the commencement of the second period of infancy, we meet with eczema simplex; and, finally, from that period up to puberty, with eczema of the scalp under its two clearly defined forms." These "two clearly defined forms" are *humid eczema* and *squamous eczema*. The first, as already stated, succeeds achor in the second period of infancy. Squamous eczema of the scalp has been described by Alibert under the name of *porrigine amiantacée*.

Prurigo and lichen are recommended to be treated with sulphurous or emollient baths. For eczema the treatment of Cazenave and Rayer is advised; and among others the following, "*yellow phagedenic wash*" of Professor Trousseau:—

"Bichloride[a] of mercury, 0·10 centigrammes.
Lime water, 20 grammes.

A yellow precipitate is produced of hydrated binoxide of mercury; two or three table-spoonfuls of this mixture are sufficient, in a glass of very hot water, for a lotion."

As Dr. Anderson's book is wholly on eczema, we may here give some account of its contents. Devergie noted 600 cases out of 1,800 as affected with eczema, and from this our author deduces the importance of his subject. He widens very much the signification of the term eczema, and strongly objects to an anatomical classification of skin diseases, which he believes to be "the most

[a] In the British Pharmacopœia, Hg Cl.

objectionable of all." He advances a view in which most of us will coincide—that the best classification is one founded upon the nature of the affection, and recommends the nosology published by Dr. A. B. Buchanan in the *Edinburgh Medical Journal* for January, 1863. The following are his conclusions as to the pathology of eczema:—

"1. That the elementary lesion of eczema is not of necessity a vesicle. 2. That it may be an erythematous state of the skin, a vesicle, a pustule, a papule, or a fissure. 3. That impetigo, lichen, and prurigo are merely varieties of eczema, in which the elementary lesions are respectively pustules and papules. 4. That cases of eczema are often met with in which an erythematous state of the skin, vesicles, pustules, papules, and fissures are met with in a combined form."

When an eczematous eruption is at its height, there are four symptoms which, according to Dr. Anderson's experience, are always present in a greater or less degree. These are—1. Infiltration of the skin; 2. Exudation on the surface of the skin; 3. Formation of crusts; 4. Itching. The exudation (2) may be produced artificially by painting over the eczematous surface a solution of potash, which not only produces a copious exudation, but also stimulates the capillary circulation, and thereby induces absorption.

In Chapter II. the *elementary lesions* are considered at some length. Contrary to the followers of Willan, our author holds that a vesicle is *not* the invariable elementary lesion in cases of eczema. He teaches that the vesicular form usually begins with an erythematous eruption, and upon this vesicles are developed. Many of these become pustules, and thus we have an assemblage of *three* elementary lesions. On page 12 an instance in point is given; and certainly Dr. Anderson's theory is not only plausible, but he has a good *primâ facie* case—one which only requires extended observation to make it come into fair professional combat with the long-established and—may we say?—*venerable* nosology of Willan.

That the elementary lesion is sometimes a papule, is also supported by an example, which, of course, could easily be multiplied; but that it is occasionally *a fissure*, rests on the *ipse dixit*, or *scripsit*, of our author.

Chapters IV., V., and VI., treat respectively of the causes, diagnosis, and prognosis of eczema. Chapter VII. is on the *constitutional* treatment, and Chapters VIII. and IX. are on the *local* treatment of eczema.

In the case of patients who, with the exception of the eruption, appear to be in good health, Dr. Anderson deprecates the use of the lancet, while he thinks local bleeding advantageous in some few cases. We confess to a strong tendency to demur to any kind of bleeding in cases such as these; it is admitted that it is unnecessary with the great majority, and on the minority no one can fairly ground an argument. Like others, Dr. Anderson recommends Fowler's solution; and very prudently he advises his brethren to adhere to one arsenical preparation, that they may the better know its use and power.

A very large variety of local remedies indicates a general failure in treatment; even so it is here. Where there is extensive infiltration of the skin, potash applications have been successfully used by Dr. Anderson, who lauds the virtues of cold water as an adjunct to their use. He very naïvely suggests the application of hydrocyanic acid in water, or glycerine, where the part is itching, "instead of *giving way to the desire to scratch.*" For our part, we believe that so long as there is itching, there will be scratching; and if the scratching is to be stopped, we must begin by stopping the itching.

Tar is spoken of as very efficacious, and is used at the Glasgow Dispensary for skin diseases, under the name of "tinctura saponis viridis cum pice." This preparation, borrowed from Hebra, to whom the author's work is dedicated, is as follows—common tar, methylated spirit, and soft soap, equal parts. He recommends oil of cade as a more elegant substitute for tar in private practice, and speaks highly of "Hendrie's Dispensary Petroleum Soap," at sixpence per cake. "This," he adds, "is one of the most delightfully perfumed soaps with which I am acquainted."

The reader will please recollect that all the prescriptions in this book, and the formulæ of Mr. Blake in that of M. Caillault, are according to the defunct Pharmacopœias, from which he must render them into "British." Dr. Frazer gives a good deal of excellent matter on the treatment of eczema. We cannot specify much of it here, but suggest consideration of his remarks on the use of cyanuret of mercury, and the value of sulphurous and alkaline preparations.

On the whole, Dr. Anderson's book is a good one, and we recommend its perusal to such as feel a special interest in its subject.

Caillault places successively after eczema, in his fifth class, lepra, psoriasis, pityriasis, and icthyosis. Without discussing the propriety of placing the last named affection in the same class with the three

preceding, we shall just enter a gentle protest against the name "ichthyosis," which ought to give way to the more correct term, ichthyiasis, and call the reader's attention to the statistical nosology lately published and circulated among the medical profession in Ireland by the Registrar-General. At page 27 of this publication we find *lepra tuberculosa* (elephantiasis Grecorum) called "the leprosy described by Moses"—possibly on the authority of Mr. Erasmus Wilson, whose statement to this effect has been described, at length, as incorrect in a paper in the last number of this Journal.[a]

As we should expect, M. Caillault makes precise and minute mention of scabies, the diagnosis of which Mr. Blake enlarges on in the foot-notes. Helmerick's ointment is recommended, and its formula is given at page 174; but Dr. Frazer's remarks are much more to the point. He doubts that the acarus is the real cause of the eruption, and states that the parasite is not always the same, as in Norway a different parasite has been discovered, though both there and here the eruption is identical. As the treatment of this pest is frequently a tedious matter, we give the following extract from page 68 "on the uses of sulphur:"—

"Dr. Belcher, in a paper published in the *Dublin Quarterly Medical Journal* (Vol. XXXIII.), states, that he found the solution of lime and sulphur now in use in the military hospitals, remove psora by a single application; and it has been equally successful in the cases in which I have tried it. It is prepared by boiling one part of quick-lime, two of sulphur, and ten of water, constantly stirring the mixture with a piece of wood until the two former unite, then decanting the liquid, and preserving it in a stoppered bottle. By boiling caustic lime with excess of sulphur, a penta-sulphide of calcium is obtained, mixed with hypo-sulphite of lime ($3CaO+12S=2CaS_5+CaOS_2O_2$). In employing this solution a warm bath is first given, then the fluid is well rubbed into all the affected parts with a sponge, for half-an-hour, depositing as it evaporates a yellow sulphurous layer; a second bath removes the stain, and leaves the patient quite cured."

We cannot furthur analyze M. Caillault's book, or notice much more of its contents. The only remaining part which strikes us as a novelty, is that on "Cachectic Diphtheria," a chronic form of our modern foe.

[a] The Hebrew, Mediæval, and Modern Leprosies Compared, by T. W. Belcher, M.D., &c.

The therapeutical appendix of Mr. Blake deserves some notice, and we have no hesitation in calling it the most practical part of his publication. Iodine, bromine, mercury, sulphur, and arsenic, with their preparations, are clearly discussed; as are also the salts of potash, soda, and ammonia, vegetable tonics, iron and its preparations, zinc and its compounds, oil of cade and creosote. Under each head we find valuable and highly practicable prescriptions, which we cordially commend to the study of our readers. The entire work is a useful one, and a practical treatise on the subject with which it professes to deal.

Dr. Frazer's book, though a little one, is not the less useful on that account. It professes to be therapeutical only, and has neither a peculiar nosology nor a full account of diseases. We have already referred to some of his remarks, and we now beg to direct the readers attention to the chapters on mercurial and arsenical preparations; to Chapter VI., on Cod Liver Oil; Chapter XII., on Baths and Mineral Waters; and especially to the "Formulary." Here will be found prescriptions for most of the preparations of any note in the treatment of skin diseases. It must be regretted that the symbolism of the *British Pharmacopœia* has not been used, instead of the symbols of former codes now abolished by law. To practitioners of standing this is merely a literary inconvenience, but to students it must to some extent prove a stumbling-block, and the sooner it be removed the better. In the next edition, which will no doubt soon be called for, we hope our suggestion will be adopted. The plan of the book is to discuss, in chapters devoted to each class, the uses and modes of application of the principal remedies in vogue for the treatment of skin diseases, and in doing so the author states his views as to the pathology and general treatment of the affections for which the remedies are recommended. As a teacher of, and a well-known writer on, Materia Medica this was not an unnatural course for him to adopt—and no doubt it has many advantages, while the very excellent index obviates any disadvantages that might arise from it. The work is pre-eminently practical in its character, and shows its author to be a sound and accomplished physician. Even the most experienced will find in it suggestions useful in every-day practice, and we strongly recommend it to our readers.

Handbuch der Lehre von den Knochenbrüchen. Von Dr. E. Gurlt, Professor der Chirurgie an der Königlichen Universität zu Berlin. Zweiter Theil. 1. Leiferung mit XXXI. in den Text eingedruckten Holzschnitten, sämmtlich nach original zeichnungen des Verfassers. Hamm: C. Müller. 1864. pp. 368.

Treatise on Fractures. By Dr. E. Gurlt, Professor of Surgery in the Royal University of Berlin. 2nd part; 1st. Fasciculus. With thirty-one Woodcuts, for the most part from original drawings by the Author. C. Müller, Publisher. Hamm: 1864. 368 pages.

In a former number of this Journal—that for August, 1862—we had occasion to notice the first part of Dr. Gurlt's *Treatise on Fractures.* We then spoke of the work as one which, when completed, would form one of those voluminous compilations which give evidence of the extraordinary toil, labour, and care, in accumulating all that is known upon any subject—for which the Germans are so justly celebrated in all departments of literature. The portion of the work now before us fully justifies such a supposition. In this goodly-sized volume of 368 pages, the author treats of—1st. Fractures of the spinal column; 2nd. Fractures of the ribs and cartilages of the ribs; 3rd. Fractures and diastases of the sternum; and, 4th. Fractures of the hyoid bone, and the cartilages of the larynx and trachea.

The same care and profound research characterize this portion of Dr. Gurlt's treatise as formed so striking a feature in the first part, or that in which the author dealt with the subject of fractures in general. It would not be possible, within the narrow limits of a review, to do justice to a work so thoroughly exhaustive as Dr. Gurlt's. We must content ourselves by enabling our readers to judge of the scope and character of the book, by attempting to lay before them the manner in which the author treats of some one subject.

In that portion of the volume devoted to the consideration of fractures of the spine, Dr. Gurlt has tabulated no fewer than 270 cases, giving a brief but complete abstract of the report of each case, as published by the surgeon under whose care it had occurred. In speaking of the treatment of fracture of the spine, the author enters at large into the discussion of the practical question as to the advantage of applying the trephine, and raising or removing the

broken or depressed portions of bone in such cases. Our readers will remember that this operative interference in fractures of the spine has found some warm advocates among practical surgeons—while others, of equal reputation, have set their faces against such treatment. Thus Malgaigne calls this operation "a desperate and a blind one." "It is not correct," he says, "to call it our only scientific resource. In every fracture with displacement the most scientific and rational plan is first to attempt reduction by the ordinary methods; and to this rule fractures of the vertebræ do not constitute an exception."

We know, however, that efforts at reduction, in cases of fractured spine, are by no means free from danger. Boyer has recorded a case in which sudden death was thus caused. Mr. South says, and we very much incline to agree with him, "that the attempt to set a fracture through the body, of a vertebra—accompanied, as it almost invariably is, with displacement, or most commonly with fracture of the vertebral arch, or articular processes—is, as Chelius observes, most highly dangerous, and ought never to be attempted." Sir Astley Cooper speaks favourably of the use of the trephine in cases of fractured spine:—"The proposal," he says, "is laudable, and the operation is not severe, nor does it increase the danger of the patient; time and experiment only can determine its value. If we could save one life in a hundred by it, we should deserve well of mankind; and if any good does ultimately result from it, Henry Cline has the merit of proposing it." As regards Cline, Sir Astley is mistaken; the proposition had been made long before by other surgeons. Brown-Séquard has, on physiological grounds, and basing his arguments, in some degree, on experimentation on animals, very strongly advocated the performance of this operation. His arguments appear to us unanswerable, and we think that a fair consideration of the recorded cases, in which the operation has been performed, justifies us in agreeing with this distinguished physiologist—"that it is high time for surgeons to put aside their fear of compromising themselves, and rather expose their reputation, than allow a man to die whom they have a chance to cure."

As regards the experience resulting from recorded instances of this operation, Dr. Gurlt has collected twenty-one cases where it was performed, and of these four recovered:—One, operated on by Alban G. Smith, and reported in the *North American Medical and Surgical Journal*, 1829; a second operated on by Edwards, Caerphilly, South Wales, and mentioned in the *British and Foreign*

Medical Review, 1838, p. 162; a third operated on by John B. Walker of Boston, 1838; and a fourth operated on by G. C. Blackman, reported in Blackman's edition of *Velpeau's Surgery*. If to these cases there are added that of Louis, published in the *Archives Géneral de Medicine*, 1836, p. 397—and that of Blair, spoken of by Sir G. Ballingal in the fourth edition of his *Outlines of Military Surgery*, 1852, p. 298—it must be admitted that an amount of success has attended the operation which should encourage others to have recourse to it in cases which appear suitable.

The nature of Dr. Gurlt's work precludes the possibility of giving our readers a full analysis. The foregoing, however, is enough to show how searchingly each subject noticed is looked into—almost every recorded case being given in abstract or tabulated.

As to the general scope and mode of execution of the work, we have already spoken in high terms in a former number of this Journal, and the present instalment of the work assuredly does not fall short of its predecessor.

NEW EDITIONS.

1. *Lectures on the Diseases of Women.* By CHARLES WEST, M.D., &c. Third Edition. London: Churchill. 1864. 8vo, pp. 687.

2. *The Science and Art of Surgery; being a Treatise on Surgical Injuries, Diseases, and Operations.* By J. C. ERICHSEN. Fourth Edition, Enlarged and Carefully Revised. Illustrated by 517 Engravings on Wood. London: Walton and Maberly. 1864. 8vo, pp. 1280.

3. *A System of Surgery.* By JAMES MILLER, F.R.S.E, &c. Edinburgh: A. and C. Black. 1864. 8vo, pp. 1387.

4. *Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England, by James Paget, F.R.S., &c.* Revised and Edited by WILLIAM TURNER, M.B., &c. London: Longmans. 1863. 8vo, pp. 848.

5. *The Pathology and Treatment of Venereal Diseases, including the Results of Recent Investigations upon the Subject.* By F. J. BUMSTEAD, M.D., &c. A New and Revised Edition, with Illustrations. Philadelphia: Blanchard and Lea. 1864. 8vo, pp. 640.

6. *The Science and Practice of Medicine.* By WM. AITKEN, M.D., &c. In two vols. Second Edition; Revised and Re-written. London: Griffin and Co. 1863. 8vo, pp. 727 and 1095.

7. *Urine, Urinary Deposits, and Calculi; and on the Treatment of Urinary Diseases.* With numerous Illustrations and Tables for the Clinical examination of Urine. By LIONEL S. BEALE, M.B., &c. Second Edition. London: Churchill. 1864. Post 8vo, pp. 439.

8. *The Nature and Treatment of Gout and Rheumatic Gout.* By A. B. GARROD, M.D., &c. Second Edition; Enlarged and Carefully Revised. London: Walton and Maberly. 1863. Post 8vo, pp. 618.

9. *Elements of Physics, or Natural Philosophy.* Written for General Use, in Plain or Non-technical Language. By NEIL ARNOTT, M.D., &c. Sixth and Completed Edition. Part I. London: Longmans. 1864. 8vo, pp. 400.

10. *Handbook of Physiology.* By WILLIAM S. KIRKES, M.D., &c. With Illustrations on Steel and Wood. Fifth Edition; carefully Revised and Enlarged. London: Walton and Maberly. 1863. Post 8vo, pp. 774.

WE have before us a number of new editions of standard works. The earlier editions of most of them have been very fully reviewed in our pages, rendering it unnecessary that we should again review them at any length; and besides, the very fact that repeated editions have been called for is stronger testimony to their merits than any that we could offer. We purpose, therefore, to call the attention of our readers briefly to the fact that new editions have appeared, and to make a few extracts, such as may be important either as expressing changes of opinion on the part of the authors, or as containing an account of new researches; or interesting, by showing the opinions held by the writers on mooted questions, and their modes of thought and expression.

1. *West on the Diseases of Women.*—This is the third edition of a work that has since its first publication been most justly esteemed a standard book. In his preface the author states that by the adoption of a fuller page he has been able to increase the matter without adding to the size of the book. The chief additions will, he says, be found under the heads of Uterine Hematocele and Ovarian Disease; but he has carefully revised the whole work, and added to it wherever larger experience has increased his knowledge.

The most important feature in this edition is the decided change in its teachings in reference to Ovariotomy. In the previous editions Dr. West entered into a lengthened argument on the merits and demerits of ovariotomy, and adduced a series of reasons for rejecting the operation, founded on the high mortality which experience and dexterity had failed to lessen; the special hazard attendant on those cases where yet the operation was specially indicated; and the utter uncertainty, even in the most favourable cases, as to the probable result. With the love for truth and the calmness of judgment so characteristic of his mind, so evident in his writings, and so attractive and influential with his readers, Dr. West now states that further experience has led him to modify his opinion, and with a true but rare appreciation of that love of consistency which has been truly called the hobgoblin of weak minds, he gives full utterance to his more matured thoughts, as will be seen by the following extracts:—

"It is between six and seven years ago since I expressed these opinions. I have thought it right to reproduce them now, word for word, and to repeat the grounds on which they rested. I have done so because these opinions are still, in the main, those of the highest authorities in France and Germany, and it is only in this country and in America that any important additional experience has been attained concerning the operation and its results.

"Even in England, most of the former opponents of ovariotomy retain the unfavourable opinion which they had already expressed, but I am not aware that anything whatever has been done, or even attempted by them to devise other and less hazardous proceedings for the cure of ovarian disease, or even for retarding its progress; and iodine injections, which seemed to promise so much, have been allowed to fall into disuse, almost without an attempt to ascertain their real value. Ovarian disease, then, remains, as far as curative measures are concerned, just where it was seven years ago; a deeper conviction of the utter fruitlessness alike

of internal remedies, and of outward applications being all that the experience of these seven years has taught us.

"It becomes, then, of the more importance to inquire whether this gloomy picture admits of no alleviation, whether the hazardous operation of ovariotomy has lost none of its dangers, whether its attempt is attended by the same uncertainty as before, and whether recoveries from it are still limited to cases where its necessity was the least urgent?

"I am bound to admit that to all of these questions the reply must be much more favourable than it was seven years ago; that the persevering efforts of the advocates of the operation have led to a greater accuracy of diagnosis; to a more careful selection of cases; to a removal of some of the dangers of the operation; to the discovery of the comparative safety of some proceedings, such as the return of the pedicle with the ligature around it into the abdomen, from which surgeons would have shrunk as nothing less than fatal, and to a more judicious after-treatment; and, consequently, that ovariotomy has increased in certainty, and gained in safety."

From an examination of the more recently accumulated facts, Dr. West shows that though completed ovariotomy is nearly as fatal as before; and that life is prolonged but little longer in fatal cases than it was before; and that the causes of death are much the same as they were, though there has been a reduction in the risk of mere hemorrhage; yet a great advance has been made in the certainty of diagnosis, as seen by the far fewer instances where the operation has been commenced and abandoned from adhesions, or some other cause interfering with its completion. He shows further that no age furnishes a positive contra-indication to its performance, and that the assumption that the only cases which furnish results sufficiently encouraging to justify the operation are cases of simple cysts, which usually increase the least rapidly, exercise the least injurious influence on the constitution, and are the most amenable to other modes of treatment, an assumption on which a chief objection to the operation has been founded, is devoid of foundation.

"The stigma of uncertainty, then, which formerly rested on the operation, may be regarded as now almost or altogether done away with; and the experienced surgeon may begin its performance without more uncertainty as to his ability to complete it than accompanies other of the capital operations of surgery.

"Neither the patient's age, then, nor the nature of the tumour, can be regarded as of itself prohibiting the operation; and we are clearly advancing into the region of certainty, when, instead of arbitrary laws

deduced from the results of a limited number of figures, we become able to lay down indications for doing or abstaining from the operation founded on the general principles of medicine and surgery.

"I think, then, that we are now bound to admit ovariotomy as one of the legitimate operations of surgery; as holding out a prospect, and a daily brightening prospect, of escape from a painful and inevitable death, which at last indeed becomes welcome, only because the road that leads to it conducts the patient through such utter misery.

"Perhaps we may sum up the indications and contra-indications for the operation somewhat thus:—

"1*st*, It is *not* to be performed in any case of single cyst which is not increasing, or is increasing but slowly, while it has not as yet interfered with the patient's general health. In other words, life is not to be jeopardized for a mere discomfort.

"2*nd*, It is *not* as a general rule to be performed until after the cyst has been tapped once. The reasons for this precaution are three-fold. In some rare cases the fluid does not re-collect; the amount of constitutional disturbance which follows tapping would be some index to the amount that might be apprehended from the more serious operation of extirpating the tumour; and lastly, when the cyst is emptied, and during the process of its refilling, its relations, and the presence or absence of adhesions, especially to parts within the pelvis, can be more readily ascertained.

"I doubt whether, in the case of simple cysts, ovariotomy ought not to be further limited to cases in which trial has been made of iodine injections sufficient to ascertain them to be inefficacious, or to prove them to be unsafe.

"3*rd*, It is *not* to be performed in any case in which a tumour is felt in the pelvis, retaining the same situation but little changed after tapping, and from which, by means of the sound, the uterus cannot be distinctly isolated.

"4*th*, It is further contra-indicated by the presence of albumen in the urine, or at any rate by the persistence of any trace of it after tapping, and also by the early occurrence of swelling of the legs, and by the presence of any considerable quantity of ascitic fluid in the abdominal cavity.

"5*th*, And lastly, its success is rendered extremely doubtful by the previous occurrence of cyst inflammation, and general peritonitis as evidenced by attacks of sickness, shivering, fever, and abdominal pain, and by the presence of pus in the fluid evacuated by puncture. The fact of a patient having had occasional attacks of abdominal pain, of short duration, unattended by fever, or by abiding tenderness, does not contra-indicate the operation, since such attacks occur independently of inflammation.

"On the other hand it is not contra-indicated,

"1*st*, By the patient's youth or age, nor by the fact of her having previously undergone several tappings, nor by the irregularity or suppression of the menses, since complete menstrual suppression does not prove both ovaries to be implicated.

"2*nd*, It is justifiable and to be recommended in all cases of ovarian tumour, whatever be its structure, and whether its existence be of long or short duration, and whether tapping has or has not been frequently resorted to, where the disease is steadily and progressively increasing, and when the patient's health is beginning to suffer from this increase, but as far as can be ascertained from no other cause independent of the local mischief.

"Something, indeed, I think much of our conduct must be governed by the state of the patient's own mind and wishes; by the calmness with which she can regard the possible failure of the operation, and the sudden entering on the 'unknown land;' by the strength of the ties which bind her to the world, and make her desirous to continue in it, and by the spirit of hopefulness that may enable her to look beyond the risk of the few days, to the perfect health in future years which will be the reward of a successful venture. Dread of the issue is a bad state of mind in which to undergo an operation of this magnitude; I am not sure but that indifference is even worse: I am quite certain that moral considerations must be weighed as carefully as those furnished by the character of the tumour, or the history of its growth.

"I cannot expect that the reasons which have seemed to me conclusive in favour of ovariotomy should appear to others equally cogent; though I quite expect that the next seven years will, as the past have done, lessen the objections to its performance, and increase the evidence in its favour. So long, however, as there continues to be room for difference of opinion on the subject, the caution is not superfluous which I ventured to suggest seven years ago with reference to the grievous injury that is done both to the advance of medical knowledge, and to the standing of our profession with the public by the practice of treating some of these questions as though they were questions of moral right or wrong. It would seem, from what has sometimes been said on the subject, almost as if ovariotomy could not be defended save from some sinister end, nor its expediency be doubted except from a moral obliquity rendered excusable only by hopeless dulness. Belief in each other's integrity of purpose seems to me essential to our eliciting truth by discussion; and I see no reason why I am to suspect another of being less mindful of our common duty to humanity because he tries to relieve suffering or to prolong life by some means in which I have not the same confidence. The *odium theologicum* has at least age and respectability in its favour; I fear the immortal quarrel between Dr. Slop and Susannah has gone far to render the *odium obstetricantium* simply ridiculous."

We now leave Dr. West's book. The whole work bears evidence of having been carefully revised, and it is well worthy of the fame it has already obtained.

The next works on our list are systematic treatises on surgery. Both Erichsen and Miller touch on this same question of ovariotomy; and though they have both advocated it before, it may not be uninteresting to place extracts from their teachings beside those we have taken from Dr. West:—

"Much discrepancy of opinion has existed amongst practitioners as to the propriety of performing this operation, which has been chiefly condemned on two grounds; 1st, that, as the disease for which it has been proposed is not necessarily fatal, or, at all events, not incompatible with long life, it is not proper to subject the patient to a hazardous procedure for its removal; and 2nd, that the mortality from the operation is so high as not to justify a surgeon in performing it.

"With regard to the first objection, it may be stated that ovarian disease is attended by very great discomfort and inconvenience in all cases; that it prevents a woman from discharging the active duties of life, and, amongst the poorer classes, from obtaining a livelihood; and that, so far from being a comparatively innocuous affection, it wastes and enfeebles the patient, interfering seriously with nutrition, and with the actions of the abdominal or pelvic organs, and is not generally compatible with prolonged existence. Dr. Lee states that about 80 per cent. of the cases that are not operated upon, die within one year and three quarters after the appearance of the disease. When it grows rapidly, or so soon as it attains such a size as to require tapping, death takes place much more quickly. Mr. Safford Lee states that of 46 patients with ovarian disease, who were tapped, 37 died, and only 9 recovered; and that of the 37 who died, more than one-half did so in four months from the first tapping, and 27 out of the 37 within a twelvemonth, and of these 18 were tapped once only. In those who survive, repeated tappings are required at constantly decreasing intervals.

"The second objection can have little weight with any practical surgeon. The mortality after ovariotomy is not so high as that after many operations which no surgeon would hesitate for a moment in performing. It is true that in some of these cases, as in the ligature of the larger arteries for aneurism, speedy death would be the penalty of non-interference; whereas, in ovarian disease, a miserable existence may be protracted for some months after the tumour has attained so great a bulk as to render surgical aid necessary. But in other surgical diseases in which operations of the gravest character are performed without hesitation, as in the removal of the upper jaw for tumour, or in amputation

at the hip-joint for tumours of the femur, the same objections would hold good. The diseases for which these great and dangerous operations are performed are not incompatible with some months or even years of painful existence; yet the surgeon does not hesitate to give the patient the chance of a doubtful recovery, in order to extricate him from the miseries of prolonged suffering. It is not necessary, in order to justify an operation even of a dangerous character, that the patient be in *immediate* peril of death. It is sufficient that the death from the disease which it is proposed to remove should ultimately be certain, though it may be delayed by months or years of previous suffering. From this point of view ovariotomy appears to me to be a perfectly proper and justifiable operation, and it presents the advantage of the cure, if practised, being complete and permanent. In it there is no mean; the death is speedy, or the cure complete."—*Erichsen's Surgery*, pp. 1234–5.

"The statistics of ovarian operation give a mortality of about one death in every three cases, though some speak of a fatal result as occurring only in the proportion of one in seven. Its dangers, then, are very considerable. On the other hand, hopes of relief from ordinary treatment of the tumour cannot be sanguine. Most patients are carried off by the disease in less than four years. Very few have the good fortune to be cured, and only a small number live beyond the four years. But it will always be a difficult and anxious matter for the surgeon to propose that a woman suffering, it may be, very little from the disease, should subject herself to the risk of almost immediate death, in order to obtain the chance of getting rid of that which might possibly permit several years of comfortable existence. The question of the performance of this operation, therefore, should not be decided so much upon general grounds, or on the statements of other operators, as by a careful consideration of all the circumstances in each particular case—favouring or forbidding its employment. The results as given by statistics are certainly not to be taken into account, as they seem to consist of the most heterogeneous materials; containing, for example, cases where small non-adherent cysts in young girls have been satisfactorily removed—cases where the bowels and the tumour were so inextricably incorporated, that large masses of the tumour had to be left behind—cases where the removal was practised carefully and skilfully—and cases where the iliac veins were torn, the bowel divided, and towels, placed in the cavity to check the bleeding, were left there, and the wound stitched up."—*Miller's Surgery*, p. 1002.

2. *Erichsen's Treatise on the Science and Art of Surgery*, from the fourth edition of which we have made one of the foregoing extracts, first appeared about eleven years ago, to have now reached a fourth

edition, besides having been translated into German and French, and passed through two editions in America, is no mean evidence of its value. The present edition is somewhat enlarged, and has 517 illustrations, being sixty-seven more than were in the preceding one. It retains its character for the completeness and accuracy of its details, and it may be referred to by the surgeon with the certainty of finding for his guidance in all emergencies practical precepts, full, and easy of comprehension. It has not, however, been brought fully up to the present day, and there are many subjects that we expected to have found fully discussed which have been only briefly alluded to, or are not mentioned at all.

3. *Miller's System of Surgery* is virtually also a fourth edition, though appearing under another title. The previous editions appeared in two volumes, entitled respectively *The Principles and Practice of Surgery*, and have also been well received abroad. These are now combined, and form a complete system. The author originally intended his volumes as text-books for his lectures, and we cannot allude to them here without a passing tribute of respect for his memory. A most eloquent and graceful lecturer, a warm and genial friend, the news of his death, which has arrived as we write, fills us with grief. Cut off at an early age, when he appeared to have a brilliant career before him, he has left us a volume which will ever form an attractive and sound exposition of the doctrines of surgery. Other works may be more minute in details and dogmatic in their precepts, but none can be more sound in their doctrines, or better suited to give students a true insight into the science of surgery. We extract the following passages from these works, with a view to showing the opinions of the authors on subjects that have lately attracted attention, and as illustrating their several styles:—

"*Opening the Windpipe in Croup and Diphtheria.*—The question as to whether the windpipe should be opened in cases of croup has been much discussed. Were croup merely a disease of the larynx, and did death in it result from simple laryngeal obstruction, it would doubtless be proper to perform this operation. But in croup there are two distinct sources of danger; 1, that arising from asphyxia dependent on laryngeal inflammation, obstruction by plastic deposits, and spasms; and 2, that which is due to the extension of inflammatory action below the larynx into the bronchi and lungs. By tracheotomy we can doubtless remove so much of the danger as arises from the laryngeal obstruction, but we cannot

remove that which is dependent on the often concomitant broncho-pneumonia. In this respect the propriety of performing tracheotomy in the croup of children differs remarkably from the expediency of having recourse to the same operation in the acute laryngitis of adults, in which the lungs are rarely implicated to a serious extent. In fact, the question as to the performance of tracheotomy in the croup of children must be answered by the amount of laryngeal asphyxia and the extent of pulmonary implication. If the child be in danger of death from uncomplicated laryngeal inflammation, obstruction, and spasm, timely tracheotomy will undoubtedly rescue it from this immediate danger, and will be a proper operation; but, if extensive broncho-pulmonary inflammation already exist, it will be worse than useless, and should on no account be practised. A serious objection to the performance of tracheotomy in the croup of young children is, that it is by no means an easy operation, or one devoid of immediate danger. If chloroform be not given, the struggles and writhings of the child will materially embarrass the surgeon in his attempts at opening the windpipe. But even if chloroform be administered, which should always be done when practicable, and this source of difficulty removed, there is, unless care be taken, no little danger of hemorrhage; and I have heard of several cases in which this has occurred to a fatal extent. Although, therefore, as a general rule, I fully agree with Porter in reprobating this in ordinary cases of croup, yet cases are doubtless occasionally met with in which the disease is so clearly limited to the larynx—the respiration being free throughout the lungs and bronchi—that the surgeon may feel himself justified in endeavouring to save the little patient, struggling against overpowering asphyxia, by opening the windpipe. In several such cases, to which I have been called by other practitioners, and which were apparently most favourable to the operation, I have considered myself justified in doing this, though rarely with ultimate success. I believe that the general experience of British surgeons is unfavourable to it. On the other hand the French surgeons, and particularly Trousseau, are strenuous advocates for the performance of tracheotomy in croup, and the operation is accordingly far more extensively practised in France than in this country. But even in Paris it is not a very successful procedure; thus it appears that, at the Hospital for Sick Children in that city, the operation was performed in 215 cases in five years, and that of these only 47 were cured. Unless we assume that the disease, as occurring in Paris, is different from the form of croup we meet with here, it may fairly be doubted whether an operation which is in itself dangerous could be necessary in many of these instances, and whether a large proportion of the children might not have recovered under ordinary medical treatment, and without having recourse to operation.

"In *Diphtheria*, as in croup, there are several sources of danger. There is not only peril from the obstruction of the pharynx and larynx, by

inflammation and the accumulation of exudation-matter, to such an extent as to induce asphyxia, but there is also liability to pulmonary inflammation and congestion, blood-poisoning, and exhaustion of the system. In diphtheria, as in croup, tracheotomy may be performed when the patient is in imminent danger of death from laryngeal obstruction; and by it the fatal event may be warded off so far as it arises from this cause, and time be thus afforded for proper treatment. Even though the relief afforded by tracheotomy be only temporary in the majority of cases, and the patients generally eventually die of the constitutional symptoms, yet it is obviously proper in cases still uncomplicated, and where the danger of death from laryngeal asphyxia is imminent, that the surgeon should rescue the patient from his instant peril, and give him his only chance of prolonging or of preserving life, by opening the windpipe,—the trachea in children, the larynx in adults, being most suitable for operation. It has been very justly observed by Dr. Jenner that, by opening the windpipe in these cases, we save the patient from so terrible a death as that by asphyxia; and even if life be ultimately extinguished by the disease, temporary ease will have been afforded the patient, and death will occur in a less distressing manner; and that, if only one life in a hundred could in this way be saved, we should be justified in having recourse to the operation.

"The prospect of saving life after opening the windpipe in cases of diphtheritic asphyxia will greatly depend upon the age of the patient. Under two or even three years of age recovery is extremely rare; as age advances, the chance of life proportionately increases, and in adults the prospect of recovery is considerable. In many cases the patient will get great temporary benefit from the operation, and will appear to be doing well for several days, perhaps for eight, ten, or fourteen; and then, to the great disappointment of the surgeon, he will die, not from the effects of the operation, but from blood-poisoning or from the extension of the disease to the bronchi and lungs."—*Erichsen's Surgery*, pp. 924–5.

"In the second stage, the symptoms are sufficiently urgent to call for any aid which our art can afford. Tracheotomy will give a more direct and free entrance for air passing towards the lungs, than through the affected larynx; and the larynx will be placed in a state of comparative rest, favourable to recovery. But the same good result does not follow as in the case of acute *œdema glottidis*. In croup, unfortunately, the disease is not limited to the larynx, but has often passed the site of tracheal wound, and is already established in the bronchial tubes; the wound is made—not in a comparatively sound part, to afford rest to the superior portion of the canal—but in the midst of the disease, affording rest to but a part, and perhaps a minor part, of the disorder's seat, and possibly inducing, by its additional stimulus, an aggravation of the whole. Air

is let in towards the lungs, but with only a doubtful chance of reaching them; for by this time the bronchial tubes may be clogged with viscid mucus, while certainly the bronchial membrane is itself swoln and infiltrated, the trachea may be more or less obstructed by false membrane, and perhaps, indeed, pseudo-membranous formation may have extended throughout almost the whole bronchial ramifications. When the third stage of croup has set in, tracheotomy must prove only exceptionally a means of restoring the patient. In this disease, therefore, the practical inference from such considerations will be, that in the first stage our principal confidence must be placed in medical treatment. When, however, the second stage has set in, in spite of medical efforts to check the progress of the disease, the question arises, Shall we perform, or shall we not resort to tracheotomy? This question has been very differently answered by the experience of different surgeons. By some it is considered as a desperate attempt to save life, and therefore not justifiably undertaken so long as medical treatment holds out a shadow of a hope of checking the disease. By others, a great success in the employment of tracheotomy in cases of supposed croup has led them to resort to its performance in every case, whenever the respiration becomes seriously interfered with, whatever the stage of the disease—so long, in fact, as the little patient is struggling ineffectually for breath and not actually moribund. To the latter class of practitioners we incline to belong, and hold that the surgeon should unhesitatingly undertake the operation whenever the dyspnœa is urgent, sustained, and accompanied by drawing in of the lower part of the chest on inspiration, with recurring paroxysmal attacks, a rapid pulse, becoming feeble, a congested or a pallid and leaden aspect, with turgid veins and copious perspiration. If the operation is not performed under such circumstances, death may ensue during the next paroxysm; or if it be delayed longer, the delay is sure to induce œdema of the lungs, to increase the likelihood, therefore, of congestion or actual stasis of the pulmonary circulation, to exhaust the heart by its inability to force onwards the blood accumulating on the right side, and lastly, still further to diminish the powers of life by the persisting dyspnœa, and the continuance of depressing treatment, so that the little chance becomes no chance at all when tracheotomy is performed at a later period. No doubt the tube and wound in the trachea may be causes of bronchitis; but certainly impending asphyxia, œdema of the lung, and its consequences, and exhaustion of the patient, are far more likely to act injuriously in determining not only bronchitis, but also collapse of the lung, if permitted to continue till a fatal issue is obviously threatened. No doubt, also, the false membrane may extend beyond the tracheal opening; that, however, does not preclude its exsputation, after tracheotomy, any more than before; nay, it saves the patient from the risk of the shreds or casts becoming impacted in the glottis, and thus

inducing instantaneous death. But experience proves furthermore that where it does not save the little patient from death, it alleviates his sufferings; affording him respite from struggling efforts, at a small expense of temporary additional pain, while it stills the hoarse cry and the barking cough which have, during the weary watch, been so sore a trial to his anxious friends. In undertaking the operation, the surgeon should satisfy himself as to the condition of the chest; seeing that the want of expansion acts equally on both sides, that the respiratory murmur—not the hoarse and shrill rhoncus communicated from the larynx and consonating through the bronchi of the lung, but of the air entering both lungs—can be heard, and that there is no dulness on percussion at either base. His duty then is plain. He should perform tracheotomy as speedily as possible. The operation may not save his patient, or the case may not be one of true croup, but certainly nothing else will help the sufferer, whether the case be croup or not."—*Miller's Surgery*, pp. 872, 3, 4.

In cases of impermeable stricture of the œsophagus it has been proposed by Sédillot to open the stomach by an incision through the abdominal walls, and to introduce food directly into the organ. The operation has been performed by Sédillot, Fenger, of Copenhagen; Cooper, Forster, and others. Sédillot has given it the name of *gastrostomy*. Our authors thus allude to it:—

"The value of this operation has yet to be determined by experience. In no case in which it has hitherto been done has it succeeded in adding much, if anything, to the prolongation of life. In reasoning upon it, two objections present themselves. First, there is the great and immediate danger of destroying life outright by the induction of peritonitis; though it is by no means impossible that the tendency to abdominal inflammation may be lessened by the previous starvation of the patient. But supposing this risk to be overcome, what is gained by the artificial opening? It is true that through it the patient might be nourished; but as every idiopathic stricture of the œsophagus is either from the first of a cancerous character, or eventually assumes a malignant action, of what advantage is it to endeavour to prolong a precarious existence, which must in a few weeks or months be cut short by the unchecked progress of a malignant disease? Would not the immediate danger of the operation much more than counteract all good to be eventually derived from it?

"There is, however, one class of œsophageal strictures which are of the most obstinate character, and rapidly fatal by simple occlusion of the tube, without any tendency to malignancy. These are the constrictions that result from the swallowing of corrosive liquids, whether acid or alkaline. In such cases as these, in which speedy death from starvation is inevitable, I think that recourse might with propriety be had to the operation of gastrostomy."—*Erichsen's Surgery*, p. 892.

"In the case of insuperable obstruction of the pharynx, œsophagus, or cardia, it has been proposed to open the stomach by direct incision; attaching the edges of the opening in the stomach to the integumental wound; and thus constituting a permanent aperture, for the introduction of food, similar to what occurred accidentally in Alexis St. Martin. The operation is feasible in theory, and simple in performance. But its extension to cases of hopeless malignant disease seems scarcely expedient."—*Miller's Surgery*, p. 1004.

4. *Paget's Lectures on Surgical Pathology.*—We need only say of this work that the author tells us, in the Preface, that when the time came for preparing a second edition of his lectures he was anxious that they should be revised with all the light of the knowledge of Pathology acquired since their first publication; and, though he had collected some materials for this purpose, yet he had not sufficient time for either studying or thinking carefully about the many facts, and probabilities, and guesses at truth which had been added to Pathology; and therefore committed the work of revision to his friend and former pupil, Mr. Turner, whom he knew to be not only very conversant with the progress of medical science, but able to test other's observations by his own; and Mr. Paget says he has so worked with him as to be equally with him responsible. The lectures are now published in a single volume, and have not only been carefully revised but have had much matter added to them.

5. *Bumstead on Venereal Diseases.*—The very favourable opinion we expressed of this book in our number for August, 1862, is confirmed by a new edition having been called for in the short space of two years. The author has spared no labour to make this edition worthy of the reputation acquired by the last, and we believe that no improvement or suggestion worthy of notice recorded since the last edition was published, has been left unnoticed. To one point we would draw his attention. In speaking of Mr. Holt's method of treating stricture, he copies from our pages Dr. P. C. Smyly's improvement in the instrument, but states that Mr. Holt directs that the catheter should be used daily after the stricture has been burst. Mr. Holt's recommendation is that it should be used every second day only; and we are convinced that the frequent introduction of a catheter is not only unnecessary, but a frequent cause of failure in this mode of treatment.

6. *Aitken's Practice of Medicine.*—This is a second edition, greatly enlarged, of a well known work, and is very correctly

described in the title page as re-written. Founded on an article written for a cyclopedia (not professional) it was soon republished in a separate form, as a handbook for students, and now, after a short period, it appears as a large two-volume book, containing upwards of 1,800 8vo pages. Dr. Aitken is professor of pathology in the Army Medical School, and was one of the pathologists attached to our army in the Crimean campaign, and as a pathologist, has deservedly a very high character. It is evident, however, from the work before us, that his attention has been directed more to pathology and the literature of medicine than to clinical research and the actual observation of disease.

As a *resumé* of the most important researches and writings, the book is one of very considerable value, affording a good digest of nearly all the recent books and essays of authority that have appeared down to the day of its publication. As a guide to the study of symptoms, however, or to the treatment of disease, it is not so trustworthy, many of the descriptions being curt and inaccurate, some important diseases not described at all, and the treatment recommended very often more of the old routine of leeches, blisters, antimony, and mercury than is judicious at the present day. In all that relates, however, to pathology, the book is most excellent; and, after all, this is the most important part of a class book, for a knowledge of the symptoms of disease and of the mode of treatment is not to be learned from books or lectures, but at the bedside only.

The same credit that we have awarded to Dr. West for his candid and unequivocal expression of a change of opinion is due to Dr. Aitken, for the manner in which he has avowed his altered views as to the nature of typhus and typhoid fevers. In the former edition he taught that these fevers were identical in their nature. This belief he says he no longer entertains; and in a very excellent and well written chapter he states the facts that have proved to him that there are many remarkable differences of a specific kind between typhus and typhoid fever, and we must add Dr. Aitken's name to the now long list of eminent pathologists who have become converts to this opinion.

Dr. Aitken devotes very considerable space in this edition to the exposition of the use of the thermometer, as an aid to diagnosis. This is, we believe, the first attempt that has been made to bring this subject forward in a systematic treatise, and to give thermometrical observations the place they deserve, as aiding the study

of disease. To Wunderlich, of Leipsic, we are indebted for his exposition and persevering advocacy of the usefulness of daily repeated observations and records of the temperature of fever patients, and the constant employment of the thermometer as a means of diagnosis at the bedside. On this subject he has written much, from an extensive experience, embracing at least half a million exact thermometrical observations, following the continuous progress of individual diseases, the results of which he has compared in more than 5,000 patients; and Dr. Aitken has embodied in his work the conclusions to which Wunderlich has arrived.

Variations of bodily temperature, through external influences, are found to indicate disturbed health. Influences which exert no effect on a healthy man, act most strikingly in certain morbid states of the body, so that the appreciation of such changes in temperature will often alone indicate latent but important disease. A mere indisposition, but attended with a considerable rise of temperature, ought never to be made light of—marking, as it usually does, the beginning of important disease. As a means of establishing a diagnosis thermometrical observations seem to be most valuable:—

"For example, the characteristic variations of the temperature, as exhibited in a typical case of enteric, intestinal, or typhoid fever, are of such a kind that they are not found in any other disease. Intestinal catarrh, severe forms of pneumonia, intermitting fever with apparent continuance, meningitis and miliary granulations of the pia mater, acute tuberculosis, Bright's disease, or pyemia, may each simulate typhus, and may exhibit some of its most characteristic symptoms; but observations with the thermometer, as to the temperature of the patient from day to day, will, at once or after a few days, establish the distinction with certainty. In the course of many diseases also, whose diagnosis has been accurately established, additional developments of disease, degeneration, or other complication, may ensue, which at first and for a time are completely hidden, if the interruption to the normal course of the disease is not marked out by a sudden rise of the temperature—the earliest and the best indication of these untoward events."

The thermometer is equally useful as a means of prognosis; and Dr. Aitken believes that it must ere long be universally adopted in practice, and in his descriptions of many diseases gives illustrations of its use. He concludes his general description as follows:—

"The student or physician who continues to disregard the aid of thermometry in the diagnosis of febrile diseases, may be compared to the

blind man guiding himself. By means of great practice and intelligence the blind man will often proceed rightly, but the advantages of being able to see clearly are proverbially above all price. The necessity of the use of the instrument, also, will soon become known to the general public, and patients will become dissatisfied if all known means of investigation are not employed in investigating the nature of their malady. For many years the German student and physician has been familiar with its use; but with the exception of Dr. Parkes, and the pupils he taught when Clinical Professor in University College Hospital, the usefulness of the thermometer, in recognizing febrile diseases, does not seem to have been hitherto sufficiently appreciated in the medical schools of this country."

It is much to be regretted that the practical parts of this work are not equal to the theoretical and scientific portions; but, notwithstanding, it is, in our opinion, a valuable addition to our class-books for students, and a most excellent compendium of all that is being done in pathology and the science of medicine.

7. *Beale on the Urine.*—The author has endeavoured to increase the usefulness of this work by the addition of observations on the treatment of urinary diseases. Experience has taught him (he says in the Preface) that the treatment of many chronic and obstinate diseases of this class is more successfully carried out by attention to the general physiological changes going on in the system, and by the use of *simple* remedies, in suitable doses, persevered in for a considerable time, than by the employment and frequent change of complex formulæ. He believes that the treatment of disease may now be conducted on recognized and intelligible principles; and that the system of ordering a number of different substances should be deprecated, because evidence has proved it to be useless to the patient, whilst it must foster mystery in connexion with our art, and greatly retard the advance of medicine. The reader will not, therefore, find a list of all the drugs that have been advocated as having a special influence on the urinary organs; nor will he meet with complex recipes containing several ingredients the action of which is very imperfectly understood.

New observations and several new figures have been introduced, and a considerable part of the work has been entirely re-written, while new matter, to the extent of nearly 100 pages, has been added.

8. *Garrod on Gout and Rheumatic Gout.*—In our issue for May, 1860, we gave a very full analysis of this work, commending it strongly. We now again advise all our readers to purchase and

study the book, believing, as we do, that it is one of the most valuable and complete that has ever appeared on the subject.

9. *Arnott's Elements of Physics.*—A book that has passed through five large editions in five years needs no commendation from a reviewer. We trust it will soon be completed, and take its place as a class-book with all who contemplate entering the medical profession.

10. *Kirkes' Physiology.*—This book also has passed through five editions in five years, and will, we have no doubt, pass through many more.

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1. *The Annals of Military and Naval Surgery, and Tropical Medicine and Hygiene; being an Annual Retrospect Embracing the Experience of the Medical Officers of Her Majesty's Armies and Fleets in all parts of the World.* Vol. I., for the year 1863. London: Churchill. Post 8vo. pp. 376.

2. *The Case of the Medical Officers of the Army Fairly Stated, in a Letter to the Right Hon. Earl de Grey and Ripon, Secretary of State for War.* By a Retired Deputy Inspector-General of Hospitals. London: C. Griffin & Co. 1864. 8vo, pp. 17.

Of the works before us the first on our list is the commencement of a new serial, to be devoted entirely to military and naval medicine, and is intended to resemble the retrospects and abstracts of Rankin and Braithwaite. It has been compiled, we understand, by Dr. Alex. Grant, of the Indian Medical Service. We have perused many of the papers, and have no hesitation in expressing our conviction that they are of a character deserving of the highest commendation, and fitted to prove that the medical officers of both branches of Her Majesty's Service are second to none in their attainments, their zeal for the promotion of medical science, and their devotion to the due performance of their duties. We had marked off several passages, meaning to extract them at length, but the space at our command is not sufficient to do them justice; and the letter of the Deputy Inspector has determined us to break through our usual rule of discussing only scientific questions, and to lay before our readers, instead, an exposition of the causes of the

very unsatisfactory state into which the medical department of the army is rapidly falling, and of the reforms necessary to restore it to efficiency.

That the condition of the medical department of the army is, at present, most unsatisfactory—that the profession has lost confidence in it—that professors and teachers are, to say the least, not encouraging good students to enter its ranks, and that there is universal discontent in all the grades of this important branch of the Queen's Service, cannot be questioned. We have only to point to the *Gazette* of the 6th July, in which thirteen acting-assistant surgeons have been admitted, without examination, for Home Service, in proof of these statements, and of the destitution to which the medical service of the army is reduced; and there can be no question that the state of this department, in its Home and Indian branches, is at present exciting very considerably the attention of the profession, the public, and the Legislature. The continual stream of letters and articles appearing in the leading medical journals of England, the discussion of the question very recently in the House of Commons, and the very unjust remarks recently made in a demi-official Government organ, all tend to show that the wave of discontent is gaining strength, and has broken at the door of the Palace of Westminster.

It would be impossible, here, to go into a discussion of *all* the causes of discontent which at present prevail in the medical service of the army; a few of the principal ones will suffice. *And first—the confidence of the medical department in its rulers has been destroyed.* The warrant of 1858, a document the result of long and patient consideration, upon the expediency and justice of which the highest medical authorities in the land were consulted, has been so mutilated, explained away, shorn of its fair proportions, that, practically, its principal and most important clauses are a dead letter. Its 17th article, especially, which touches the social position, privileges, and comforts of the medical officer is either practically ignored or wilfully misinterpreted. Surely it is no unreasonable request that this warrant, which was granted after careful deliberation, should be restored in its integrity.

It has been said that medical officers wish the complete restoration of this warrant simply because they desire executive rank, and wish for military command. There may be a few foolish men in the ranks of the medical service, who may be ambitious of these follies, but we are persuaded they are few, and that

the great majority seek only for fair dealing. They naturally desire that the provisions of the warrant which they have received from their Queen shall be interpreted literally, and carried out honestly.

Another serious cause for discontent is the present almost hopeless prospect of promotion for the assistant surgeon. Let the students who are thinking of entering the army ponder it well ere they take the step. Between fifteen and twenty years must elapse before those who enter the service now can be promoted. This, on even the highest pay of thirteen shillings a-day, is but a poor prospect.

It is surely no great demand, under such circumstances, that the assistant surgeon shall receive his promotion within a limited term, say ten years, whether there be a vacancy or no.

Again, the period of service necessary to entitle a medical officer to his retirement is much too long. It is but a very few who attain now to the higher inspectional ranks; the great majority retire as surgeons-major, at twenty-five years service, on the pay of seventeen shillings a-day; too old to commence any active employment by which they could add anything to their small income; too shattered by the wear and tear of long residence in bad climates to be able to call up even the shadow of the energies of younger days; too wearied with the "rack of this rough world" to commence the battle of life again. A retirement after twenty or twenty-one years is no very unreasonable demand from men who expend their energies in every quarter of the globe, and few of whom ever reach their period of service without damaged, if not broken constitutions.

The discontent in the military medical service has, of late, taken such hold upon the profession that it has been with very great difficulty that candidates could be found to enter the service; and at this moment there are upwards of 200 vacancies in the Indian and Queen's Services. The step, however, which has been taken within the last few weeks to fill up these vacant places has been the heaviest blow ever inflicted upon the rights and liberties of the Army Medical Department. When candidates could not be found to come forward to compete for the vacant appointments, and enter the service in the legitimate channel, it was determined to admit them *without competition* and *without examination;* and an advertisement was issued in the *Times*, signed by the Director-General, offering ten shillings a-day and *home service* to any gentleman, holding the usual qualifications, up to the age of forty years,

who wished for such employment; and in a recent *Gazette* no less than thirteen of these acting assistant-surgeons have been appointed.

Now we have no hesitation in saying that the gentlemen who accept these appointments, as acting assistant-surgeons, are doing an act of great injustice to the regular medical officers of the army. Observe. These gentlemen are to be employed entirely on home service, every assistant surgeon, and many staff surgeons also, are to be banished from home to make room for them. In a short time, if this advertisement be acted upon, scarcely an assistant surgeon of the staff will be left in the three kingdoms; and if the arrangement continues they will have but a poor chance of returning home again. The immediate effect, then, of the employment of these gentlemen, as acting assistant-surgeons, will be the expatriation of the regular assistant surgeons now serving at home, whose hope of spending even a few months on home service will thus be blighted at once.

Again, there can be no question that the medical department of the army is insufficiently paid. What medical practitioner in civil life, we would ask, would consider that he had gained even an average amount of success, who, after struggling for fifteen years, found himself in the possession of £320 a-year.

The truth is, the price of a well qualified medical man has gone up. The article is much more scarce than it used to be, and if the Government want to have good men, who can be trusted, they must pay for them. Lord Hartington, in his recent speech on the subject, acknowledges this, and admits that the necessity of increasing the pay of army medical officers may arise ere long.

There are many other matters of minor importance upon which we could dwell, which are galling and painful to army surgeons. The order for *directing* the branding of soldiers, a matter which, we apprehend, no more requires the presence of a mediacl officer than the shoeing of a horse requires the presence of a veterinary surgeon; it can be done equally well by the drummer or the farrier. It is galling and humiliating to men who have been taught, in the exercise of their humane calling, never to spill blood, or inflict pain, except for the cure of disease, to be obliged to stand by and *direct* (for this is the wording of the order) the piercing of a culprit's skin with a number of holes, in the form of the letter D. or B. C., and to have the same rubbed over with a solution of *gunpowder!* Why not send our army surgeons, at once, for a course of instruction in tatooing among the Feejee islanders? for we apprehend there is not a man among the ranks of the medical department who would

not feel ashamed to confess that he was *capable* of giving instruction in the execution of this obnoxious duty.

It is a grievous pity that for the want of a few concessions that this service has become so unpopular. We have no doubt, whatever, that if a wiser and more liberal policy were adopted the medical department would be as popular as any other branch of the service. We would gladly see the day when it would be as difficult to obtain an appointment, by competitive examination, in the Army Medical Department, as it is now in the Royal Engineers; when instead of lowering the qualifications, extending the age, and, as a last refuge, admitting rejected candidates as acting assistant-surgeons for *home* service, it would be found necessary to do the very opposite, and to make the examination so searching that none but men of a high order should be admitted. But this day will not arrive till the Government see fit to grant the fair and just requirements of the medical officers.

We have observed, with much pleasure, that the College of Physicians in Edinburgh, and the Faculty of Physicians and Surgeons of Glasgow, have taken the matter up, and by a memorial to the Premier have pointed out the condition of the army medical service, and respectfully invited his attention to the subject. We trust this example may be followed by other corporate bodies, for, after all, it is to the profession in general, to the colleges, and to the medical press that the army surgeons must look for assistance in having their case fairly stated, and influentially represented.

We ask our readers to peruse the temperate, calm, and argumentative letter of which we have given the title above, and then wonder, as we do, how it is that a policy, which has tended to lower and degrade an honourable service, and to insult a profession esteemed by men among the highest, can thus be wantonly pursued. Surely such a policy must be suicidal as it is short-sighted. Who can tell, in these days, when we may not be plunged, once more, into war. Are we to have a repetition of the old story; is Scutari to be acted over again?

This letter contains the whole question. After showing the present condition of the service, pointing out the continual opposition, extending over a long series of years, which has been given to its advancement, and quoting the memorable words of Lord Dalhousie, condemning "a system which treats a member of a learned profession, a man of ability, skill, and experience as inferior to a cornet of cavalry; a system, in fine, which thrusts down grey-headed

veterans below beardless boys." The Deputy-Inspector winds up his admirable statement with the remedies which he proposes for the existing evils, which we quote, and in which we fully concur:—

"Let, then, the Warrant of October, 1858, and Clause 17 in particular, be restored in all its integrity; let the Government give a simple assurance that in future it will repress firmly all attempts, covert or open, to explain away by circulars or alterations in the Queen's Regulations its plain, honest, and obvious meaning and intention. If any Medical Officer, presuming on this clause, should be silly enough to put forward any the smallest claim to military command, your Lordship may rely with confidence on the Queen's Regulations and the Mutiny Act in the hands of military commanders to repress such pretentions should they arise, without having recourse to exceptional regulations injurious and affronting to the whole department.

"Let such pressure be put on the India Office by Her Majesty's Government as shall insure the extension of the Warrant in its integrity to India.

"Make retirement optional on a fair pension after twenty years. The nature of the military medical service is so trying to health that many after that time, for the most part spent in unhealthy climates, are incapable, without much risk and suffering, of further duty.

"Make retirement at twenty-five years also a matter of right, at the full rate of pension for this period of service.

"Let a few simple alterations in the dress of Medical Officers be made, so that, while there will be no difficulty in recognizing them as members of the Medical Staff, what is offensive in the details of the present dress may be done away with.

"Let the pay of Medical Officers, beginning say at 12s. 6d. *per diem*, increase in some regular progression at stated periods until the rank of surgeon-major is attained, with the present pay of 25s. a day.

"Finally, let assistant-surgeons be promoted at the end of ten years' full pay service.

"These measures will keep hope and energy alive in the service, banish discontent, restore confidence in the Schools, and promote the highest interests of the public service."

P.S.—As these sheets are passing through the press we learn that Sir C. Wood brought forward, on 19th July, an India Medical Service Bill, that would do away with competitive examinations, which he declares have entirely failed for the Army Medical Service. By this means the vacancies in the service may no doubt be filled up, but what safeguard are we to have against the admission of such men as the rejected candidates lately described by Professor Parkes in his speech to the Medical Council.

1. *The Prescriber's Companion.* By Alfred Meadows, M.D., M.R.C.P. London: Renshaw. 1864. Pp. 152.

2. *The Prescriber's Analysis of the British Pharmacopœia.* By. J. Birkbeck Nevins, M.D., London. Second Edition. London: Churchill. 1864. Royal 32mo, pp. 264.

3. *The Prescriber's Pharmacopœia, containing all the Medicines in the British Pharmacopœia of* 1864, *arranged in Classes according to their Action; with their Composition and Doses.* By a Practising Physician. Fifth Edition. London: Churchill. 1864. Fcap. 16mo, pp. 156.

4. *Selecta è Præscriptis: Selections from Physicians' Prescriptions; containing Lists of the Terms, Phrases, Contractions, and Abbreviations Used in Prescriptions, with Explanatory Notes; the Grammatical Construction of Prescriptions; Rules for the Pronunciation of Pharmaceutical Terms; a Prosodiacal Vocabulary of the Names of Drugs, &c.; and a Series of Abbreviated Prescriptions Illustrating the Use of the preceding Terms: to which is added a Key, containing the Prescriptions in an Unabbreviated Form, with a Literal Translation. For the Use of Medical and Pharmaceutical Students.* By Jonathan Pereira, M.D., F.R.S. Fourteenth Edition. London: Churchill. 1864. 24mo, pp. 352.

5. *Notes on the British Pharmacopœia; showing the Additions, Omissions, Change of Nomenclature, and Alterations in the Various Compound Preparations; with the Doses of those Medicines which are comparatively New.* By A. F. Haselden, Pharmaceutical Chemist. London: Hardwicke. 1864. Pp. 82.

6. *The Essentials of Materia Medica and Therapeutics.* By Alfred Baring Garrod, M.D., F.R.S. Second Edition. London: Walton and Maberly. 1864. Small 8vo, pp. 391.

7. *A Treatise on Pharmacy. Designed as a Text Book for the Student, and as a Guide for the Physician and Pharmaceutist; containing the Officinal, and many Unofficinal, Formulas, and Numerous Examples of Extemporaneous Prescriptions.* By Edward Parrish, Member of the Philadelphia Coll. of Pharmacy, &c. Third Edition. Philadelphia: Blanchard and Lea. 1864. 8vo, pp. 850.

The appearance of the revised *Pharmacopœia* of the United States of America in the latter part of the year 1863, and of the *British Pharmacopœia* early in the present year, has had the effect of communicating a fresh impulse to the energies of an ever-fertile press, the operation of the specific stimulus having manifested itself in an unusually abundant issue of new works on medicines and medicinal agents. Some few of these volumes, whose titles have been placed above, are now before us, inviting a careful perusal, and awaiting a candid criticism. One might be induced, *à priori*, to expect that but little variation would present itself in a series of commentaries from different pens on the several preparations of our Pharmacopœias, save that which would necessarily depend on the peculiar arrangement of the mere details; one writer preferring to tabulate his materials in accordance with his views of their therapeutic action; another taking advantage of an alphabetic arrangement; while a third proceeds upon a plan which differs from either of the others; and so on. Supposing, for example, that we desired to obtain some information concerning a new medicine or preparation in the *British Pharmacopœia*, and one whose name was not to be found in any of the older formularies, we are reasonably led to expect that, in their general features at least, the several accounts or descriptions of the same medicine will agree, inasmuch as the *Pharmacopœia* furnishes an unvarying and authentic basis whereon the several writers may found their comment or their criticism. Let us proceed to examine this point. On casually glancing at one of the indices we are arrested at the name of *syrupus ferri phosphatis*, and on referring to the proper page we read:—"Ferri phosphas.—Prepared by mixing a solution of sulphate of iron with one of phosphate and acetate of soda;" and a little lower down we are informed that the *syrupus* is to be "prepared by adding to the well-washed precipitated phosphate, prepared as above, some dilute phosphoric acid, and lastly sugar. 224 grains of the *original sulphate* of iron are *contained* in twelve fluid ounces of the syrup, or about *two* grains in the drachm" (Meadows, p. 18). Now the merest tyro in chemistry must perceive that not a particle of the original sulphate is contained in the syrup of the phosphate, if the latter be made as directed in the *Pharmacopœia*. We turn next to Dr. Garrod's work, and find, at page 75, the following account of this preparation:—"Phosphate of iron is prepared by precipitating a solution of 224 grains of granulated sulphate of iron, with a mixture of 200 grains of phosphate, and 74 grains of acetate of soda; the precipitate is pressed strongly

between folds of bibulous paper, and five fluid ounces and a half of dilute phosphoric acid added; as soon as the precipitate is dissolved the solution is filtered, and sugar added and dissolved without heat. *One grain* of the iron salt is contained in each fluid drachm of the syrup." As the exact quantity of each of the chemical materials employed is here set down, the student is furnished with a ready means of calculating for himself whether the strength of the preparation has been correctly stated; and he will find that when the protoxide of iron, derived from the original sulphate, has combined with phosphoric acid, acording to the formula $3FeO\ PO_5$, a precipitate will be formed, which (supposing it to be free from moisture) ought to weigh about *ninety-six* grains. This precipitate, when dissolved as directed, becomes ultimately diffused through *ninety-six* fluid drachms of the syrup. On referring next to page 28 of Dr. Nevins' *Analysis*, we find this same preparation stated to contain, of "phosphate of iron, dissolved by excess of phosphoric acid, *three grains and a half* in each fluid drachm." According to the *Prescriber's Pharmacopœia*, "one fluid drachm contains one grain of *salts*." With a view of ascertaining whether the discrepancies found in the several accounts of this preparation might not be attributed to accidental inaccuracy, we proceeded further to examine another of the iron preparations, namely, the syrup of the iodide, and expected to find more unanimity of opinion. The following is the result:—Dr. Meadows states that this syrup "is about twice the strength of the preparation in the *Pharmacopœia Londinensis*, and contains about *ten grains* of the crystallized iodide in a fluid drachm;" while Haselden considers that "one fluid drachm should contain about *five* grains of iodide of iron." Dr. Garrod, with a very near approach to accuracy, estimates the strength of the preparation as four grains and a half in the same quantity; Dr. Nevins, one grain in every twelve drops; *Prescriber's Pharmacopœia*, four grains and a half to the fluid drachm. In the directions of the *British Pharmacopœia* for forming this preparation, there is a source of error which might easily be overlooked in making the necessary computation of its strength; this is contained in the statement, the "product should *weigh* two pounds eleven ounces," while in fact it measures at the same time about thirty-one ounces, as may of course be ascertained by dividing the weight by the specific gravity. According to our own calculation, the strength of this syrup is, *quam proxime*, 4·3 grains in each fluid drachm.

The arrangement adopted by Dr. Meadows in his *Companion* is rather a good one, and offers many advantages in the way of reference. The little work is divided into six parts. The first contains remedies for external application; in the second are arranged those medicines which act upon the blood; the third comprises such as act on the nervous system; the fourth contains those which affect the secretory system, while in the fifth are found medicines whose action is exerted on the generative system; the sixth contains a list of disinfecting agents; also poisons, and their antidotes. The author's intention, as stated in the Preface, has not been in any way to give directions how to *make*, but only how to *use*, the preparations—to state their composition, and to give briefly an outline of the process adopted in the manufacture of some of them. There are, however, throughout the work numberless instances in which this intention appears to have been in some measure lost sight of; and even in the attempt to transcribe the names and descriptions of articles of the Materia Medica, from the pages of the *British Pharmacopœia*, the errors are neither few nor unimportant. *Sarsa* in one place and *sarza* in another are described as the *powdered* root of smilax officinalis, on the authority of the *B. P.* Ammonium is confounded with ammonia, potassium with potassa; nominative cases are mixed with genitive in the most perplexing confusion, and without the slightest regard to grammatical propriety. Again, the seeds of the amygdalus communis are ordered by the *British Pharmacopœia* to be prescribed under the name of "amygdal*a*," in the *singular* number, like rosa, colchici semen, &c.; our author transcribes "amygdal*us*," as if it were thus written, although in the next page he adopts the name oleum amygdalæ without hesitation. By the way, these two substances, namely, the seed and the oil, we find classed under the head of sedatives! the dose of the oil being stated to range from one to four fluid drachms as a demulcent and emollient; from one to two as a sedative. One would suppose that a little more trouble would be bestowed upon cautioning the student against confounding this oil with the essential oil of bitter almonds. Now, the *British Pharmacopœia* describes oleum amygdalæ as being "pale yellow, nearly inodorous, or having a nutty odour, with a bland oleaginous taste;" and has carefully supplied a test to be used in examining the seeds from which this same oil is expressed—"not bitter; not evolving the odour of bitter almonds when bruised with water." This would appear to exclude the *sedative* element altogether. Again, in

commenting on some of the liquid extracts—a class of preparations whose introduction into our new *Pharmacopœia* cannot be too highly commended—they are simply designated aquo-spirituous extracts, a misnomer in more ways than one; while in the case of extractum ergotæ liquidum, the object or intention of employing ether in its preparation is quite misinterpreted, whereby a student would be led astray in his search for information regarding a form of medicine which promises to be of much value.

We were just about closing this book, and bidding a long farewell to *The Prescriber's Companion*, when the following passage in the Preface caught our eye:—"Perhaps the simplest way of indicating the changes *now* and *hereafter* to be *adopted in prescribing* is by giving an illustration, for which the following may suffice:—

"℞—Ammoniæ carbon*as*, grs. 30.
Spirit*us* chloroformi, f ʒi.
Aqu*a* camphoræ, f ʒviii.—M. ft. mist.

"The difference in the *numerals* used respectively for solids and fluids is in accordance with the order of Council."

Should this page meet the eye of some student preparing for examination, amongst which class we regret to say are to be found some who are not very expert at latinising their therapeutic intentions, we would earnestly recommend him to compare diligently the production which is copied above, with some of the excellent formulæ contained in the ninth chapter of Dr. Pereira's *Selecta è Præscriptis* (Fourteenth Edition, pp. 149 et seqq.). It were no waste of time, indeed, to bestow a careful perusal on the entire chapter, which is entitled, "On the Grammatical Construction of Prescriptions." If he should fail to find himself, on completing this task, prepared to indite something more creditable than the specimen here placed before him, he must be lamentably dull and deficient.

Of a different stamp altogether from the foregoing is the little work of Dr. Nevins; differing in its arrangement—the contents of the several chapters being disposed alphabetically—different in the value of its contents, the information which it contains being, with one or two exceptions, remarkably accurate, clear, and to the purpose. There is one chapter to which we desire particularly to direct the attention of students and young practitioners, namely, that which gives a detailed account of the new substances and preparations introduced into the *British Pharmacopœia*, as well as an account of

several medicines in modern use which are not contained in our National Code. This chapter extends to nearly 70 pages, and will be found to repay a diligent perusal. Towards the end of the book there is a clear and happy exposition of the *rationale* of the volumetric processes recommended by the *B. P.* to be employed in testing the strength of the several chemical preparations. To one unaccustomed to volumetric manipulation there is some difficulty and obscurity in the Pharmacopœial directions for preparing the test-solutions; nothing, however, can be more lucid and intelligible than the exposition which Dr. Nevins has given of the entire subject in this final chapter. On the whole, we consider this analysis of the *Pharmacopœia* to be a most excellent and useful *vade mecum*.

The *Prescriber's Pharmacopœia*, by a Practising Physician, has been known to the profession for upwards of thirteen years, during which time four editions have successively been issued. The plan of the present edition appears to be similar to that of its predecessors; remodelled, however, in accordance with the new *Pharmacopœia*. We are informed, moreover, in the advertisement, that the whole of its contents have been otherwise carefully examined and corrected; and truly a diligent scrutiny of the entire work enables us to attest the accuracy of this statement. Errors of haste or inadvertence, from which books of this kind can be hardly expected to be quite free, are apparently but few here; nevertheless there are some which we cannot avoid to notice. In one place we read:—"Linimentum calcis (carron oil). *Comp.* Lime water and olive oil, of each equal parts." In another place:—"Linimentum calcis (carron oil, not officinal). *Comp.* Equal parts of lime water and linseed oil." Again we read:—"Lotio flava (aqua phagedenica, not officinal). *Comp.* Hydrarg. *bichlor.* 2 fluid grains, aquæ calcis 1 fl. oz.;" and further on in the same page:—"Lotio nigra (black wash, not officinal). *Comp.* Calomel 20 gr., liq. calcis 6 gr." It is easy to trace the origin of these errors, but we cannot so readily account for extractum lupuli being designated "a watery extract." In apportioning the doses of medicines throughout this book, the author has acted upon an opinion which is strongly expressed in the advertisement to his first edition, to the effect that "the general habit of practitioners in this country is to prescribe medicines in doses not merely unnecessarily but injuriously large;" accordingly, the doses in many instances are represented rather below the usual standard, at least as regards their minimum. By the way, there is one of our new medicines, namely, digitalinum, whose ordinary dose

is represented as ranging from one-fortieth to one-twelfth of a grain, which would appear to be rather large, for the present at least, and until we shall have had a little more clinical experience of its action on the system.

Of the late Dr. Pereira's *Selecta è Præscriptis* it were almost superfluous to speak. It is a work which has been before the public for forty years, the first edition having been published in 1824. The following extract from the preface to the fourteenth edition, which has recently appeared, may be taken as a guarantee that this is not a mere reprint:—"By the publication of the *British Pharmacopœia* certain changes have been effected, especially with reference to weights and measures and the names of medicines, and these and some other similar changes it was necessary to notice. The object, however, of this little work is not merely to represent the prevailing mode of prescribing medicines according to the instructions of Pharmacopœias which continue in authority, but to explain and illustrate the use of terms which are commonly used or may be occasionally met with in extemporaneous prescriptions, and a knowledge of which is required alike by medical and pharmaceutical students. The prescriptions contained in the second and third parts of the work are intended to represent such as are met with in practice, and in which old as well as modern names and other terms are employed."

In order to meet the requirements of the new *Pharmacopœia* it was necessary that the chapter on the symbols and signs used in prescriptions should be newly constructed—in fact altogether re-written. This has been so ingeniously done, by cancelling a few leaves of the last edition and substituting fresh ones in their stead, that the pagination remains exactly as before. In the second and third parts of the work we have searched carefully among the prescriptions for the names of medicines whose old names have now acquired a different meaning, as in the case of hydrargyri chloridum, which formerly (in England) meant calomel, but now represents corrosive sublimate; and in every instance, on comparing the present with the last edition, we have found the necessary alterations have been made. The character of the book appears to be thoroughly maintained in this new edition, a copy of which ought to be in the hands of every student.

The *Notes on the Pharmacopœia*, by Mr. Haselden, will be found to contain, in a concise and well-arranged form, a series of tables in which are pointed out the additions, omissions, and changes of name

which have occurred as regards the *British Pharmacopœia* when compared with the last editions of those of London, Edinburgh, and Dublin. As a convenient book of reference for practical pharmaceutists it promises to be very useful, the tables being most carefully and elaborately constructed, and extending over six-and-thirty pages. After these tables come the "Notes," which consist of a short review of each of the pharmaceutical preparations separately, showing the actual difference existing between the present and former preparations bearing the same or similar name. The dose, in a great many instances, has been omitted, which, in our opinion, detracts somewhat from the value of the book. In the case, however, of all medicines which have been materially altered, or are comparatively new, the dose is carefully stated. As we have alluded to digitalinum in speaking of another of these little guide books, we now refer to the same word, and transcribe the note thereon:—"Digitalinum.—This very powerful alkaloid (?) is introduced for the first time; its employment requires the greatest possible caution, the dose being one-sixtieth to one-thirtieth of a grain. The process resembles that given in *Parrish's Practical Pharmacy*." We have examined this book very carefully, and have no hesitation in recommending it as a safe guide to the student.

The *Essentials of Materia Medica and Therapeutics* in its present shape is a very considerable modification of the former edition of a work, which appeared under the same name, from the pen of Dr. Garrod. Before proceeding to examine the work in detail we should lay before our readers the announcement which faces the title page of this second and much enlarged edition:—"It has been considered desirable to make two separate books of the work originally announced under the title of *Medicines; their Nature and Value in the Treatment of Disease*. The *Essentials of Materia Medica and Therapeutics* is now published, a work of the kind being much in request at the present time. It will be followed by a companion work, completing the original idea, and devoted exclusively to the consideration of the value of medicines in the treatment of disease. Each book will be complete in itself."

We are further informed, in the Preface, that "the present work is intended to serve as a text-book of Materia Medica; and, while it is hoped that it omits nothing essential to the study of the science, it excludes such details as are often embarrassing to the student and seldom necessary to the practitioner. It has been his object, while limiting its size, to include all points connected with

the officinal preparation of medicines, and so much information on the therapeutic action of drugs as would serve as a sufficient guide in actual practice. All controversial points have been avoided as unsuited to the design of the work, and the information confined to the facts really ascertained as to the action of each drug, and the purposes for which it has been advantageously employed."

The work opens with a table, in which are exhibited the more important differences between the nomenclature of the *London Pharmacopœia* of 1851 and the new British. This table also shows those alterations in the strength of preparations which have appeared to possess sufficient importance to render them *essential* to be borne in mind in prescribing. It is much to be regretted that the author has omitted the consideration of the essential differences between the *British Pharmacopœia* and those of Edinburgh and Dublin respectively; and we cannot avoid contrasting the want of completeness of this table with the extreme fulness of those given in Mr. Haselden's book above noticed.

No work on medicines can be complete without a clear and comprehensive statement of pharmacopœial weights and measures, and their relations to other weights and measures; and, accordingly, the author has prefixed to this treatise a very careful *resumé* of this department. In the course of his remarks he observes:—"It will be observed that *symbols* representing the different weights and measures have been introduced—℔. representing the pound, oz. the ounce, and gr. the grain. It will also be seen that the numbers made use of under the weights are of the *Arabic* character. The symbols for the measures are—C for the gallon; O, the pint; fl. oz., the fluid ounce; fl. drm., the fluid drachm; and min., the minim; and the numbers, instead of being Arabic, as in the case of the weights, are the Roman numerals. It will be seen that the solid drachm and the scruple have been omitted from the *Pharmacopœia; if still made use of* they will represent sixty and twenty grains respectively in Great Britain, and not the eighth and twenty-fourth part of the avoirdupois ounce; but in Ireland these same symbols will only represent 54·68 and 18·22 grains. Such being the case it will be advisable to discard the employment of these symbols altogether. The avoirdupois fluid ounce corresponds to the solid ounce in the case of distilled water at 60° Fah.; that is, one fluid ounce weighs exactly an ounce. A minim of distilled water, however, does not weigh one grain, as the fluid ounce is divided into 480 minims, the solid ounce into 437·5 grains only."

There are some matters connected with the use of these symbols which we have not seen noticed by any of the numerous commentators and writers, and to which we cannot avoid directing attention. If the symbols for the several measures and weights are to be employed by prescribers according to the example furnished by the *Pharmacopœia*, it may be fairly asked: How are we to represent half a grain of any solid medicine? On referring to the table of weights in the *Pharmacopœia* the only symbol for this quantity is met with in the case of the ounce, which is stated to contain 437·5 grains. This would appear plainly to indicate that ·5 is the correct representation of one-half; and the fact that all through the *Pharmacopœia* the employment of vulgar fractions has been carefully eschewed, is in favour of the view we have expressed. It may be objected that ·5 could easily be mistaken for the whole number 5, and thereby a dangerous or even a fatal consequence be induced; but such an objection is easily removed by the prescriber taking care to prefix a cypher in every case where a quantity less than unity is employed; thus, half a grain would be, gr. 0·5; one quarter grain, gr. 0·25. There may be some doubts, also, whether in the case of fluids the old symbol for one-half should remain in use, seeing that no provision has been made for such a quantity in the *Pharmacopœia*. No doubt it was a most convenient sign, and not very liable to be misinterpreted, while it conforms well with the Roman numerals, which are recommended for liquid measures. We have met with many practitioners who are disposed to look upon the introduction of such makeshifts as "fl. oz." and "fl. drm.," as a questionable improvement on the old-fashioned symbols for an ounce and a drachm; to such we would recommend the excellent suggestion contained in the following extract from a work by Chamberlain, who wrote in 1819. He says:—"I wish some other character could be substituted either for the *ounce* or the *drachm*; for the two characters being so extremely alike, I am well persuaded many very bad mistakes occur; the *drachm* being nearly a figure of 3, requires nothing but one small angle over it to make it an *ounce*; thus, the mistake is easily made, in the hurry of writing by the prescriber, or in the making up by the compounder. So sensible of the possibility, or rather so well acquainted with the certainty of fatal mistakes having happened from this similarity of figure in the marks of the drachm and the ounce, was that excellent and incorruptible patriot, Dr. Charles Lucas (of Dublin), that the very first bill he brought into Parliament, after he had been returned a

member for the City of Dublin, was a bill to compel the physicians of Ireland to discontinue the using of characters in their prescriptions, and to write the words at full length—*uncias tres, drachmas duas, scrupulum unum cum semisse.*"—(*Tyrocinium Medicum*, second edition, as quoted by Pereira.)

The remainder of the introductory chapter is occupied with some general remarks on certain groups of pharmaceutic preparations contained in the *British Pharmacopœia.* Concise as these remarks are, they will be found of much service, the reader being enabled at once to discover the nature of the more important general characters, and readily to comprehend the peculiarities in nomenclature and methods of preparation found in many of these preparations. The information conveyed in this chapter is presented in a much more readable shape than it would probably be by the aid of tables.

In the arrangement of the MATERIA MEDICA, substances of inorganic origin are first treated of in regular succession; next, medicines obtained from the vegetable kingdom, classed according to the natural system, commencing with plants highest in the scale of organization; and lastly, animal products, in conformity with a similar arrangement. In order to give a correct view of the general way in which the subjects have been treated in the work before us, we shall transcribe *in extenso* the article on Acidum Hydrocyanicum Dilutum, as a fair specimen:—

"Acidum Hydrocyanicum Dilutum. Dilute Hydrocyanic Acid, or Prussic Acid. Hydrocyanic acid dissolved in water, and constituting 2 per cent. of the solution.

"This acid contains rather more than half as much anhydrous acid as Acidum Hydrocyanicum.—*Edin.*

"*Prep.* (Ferrocyanide of potassium, two ounces and a quarter; sulphuric acid, seven fluid drachms; distilled water, thirty fluid ounces, or a sufficiency. Mix the acid with four fluid ounces of the water, and to these, placed in a retort, when they have cooled, add the ferrocyanide of potassium, first dissolved in half-a-pint of the water. Put them into a retort, and adapt this to a receiver, containing eight ounces of the water, which must be kept carefully cold. Distil with a gentle heat till the fluid in the receiver measures seventeen ounces; lastly, add three ounces or as much water as may be necessary to bring the acid to the required strength.) The changes occurring in this process are rather complex, but in substance may be shown as follows: Ferrocyanide of potassium is a double cyanide of potassium and iron (2 K Cy+Fe Cy+3 HO); when acted upon by

sulphuric acid and water, a portion of the cyanide of potassium of the salt is decomposed into hydrocyanic acid and sulphate of potash, and a salt containing two equivalents of cyanide of iron to one of cyanide of potassium, is left in the retort, called Everitt's yellow salt; the formula for the decomposition is as follows:

$$(K_4Cy_4+Fe_2Cy_2)+3\,(HO,SO_3)=(Fe_2Cy_2+KCy)+3\,(KO,SO_3)+3\,H\,Cy.$$

"Anhydrous prussic acid can be prepared by passing sulphuretted hydrogen (hydrosulphuric acid) over cyanide of mercury: a sulphuret of the metal and hydrocyanic acid are then formed. Hydrocyanic acid is also produced when amygdaline, a principle contained in the bitter almond and the kernels of allied fruits, is decomposed by the action of the albuminous principle contained in such fruit; *vide* Amygdala Amara.

"*Prop. & Comp.* The dilute acid, prepared as above, is a colourless liquid of peculiar odour and taste, entirely volatilized by heat, with a very slight acid reaction, and the reddening produced on litmus paper fugitive in character. Sp. gr. 0·997. It contains 2 per cent. of real or anhydrous acid (H, Cy). Treated with a minute quantity of a mixed solution of sulphate and persulphate of iron, and afterwards with potash, and finally acidulated with hydrochloric acid, it forms Prussian blue. With nitrate of silver it gives a white precipitate, entirely soluble in boiling nitric acid. Half a fluid ounce of the acid, when treated with an excess of solution of soda, requires the addition of 80·66 measures of the volumetric solution of nitrate of silver before a permanent precipitate begins to form, which corresponds to 2 per cent. of anhyhrous acid. The explanation of this test is as follows: When nitrate of silver is added to a solution of hydrocyanic acid with excess of soda, a double cyanide of silver and sodium is formed, which is soluble, and is dissolved when the solution is well stirred; this salt is formed until all the hydrocyanic acid present is exhausted, and then the further addition of nitrate of silver causes the precipitation of the oxide of silver, which is insoluble. As the double cyanide has a definite composition, by knowing the amount of nitrate of silver required to produce a permanent precipitate, we can calculate the amount of hydrocyanic acid present in the fluid. The decomposition may be illustrated by the formula, $Ag\,O,\,NO_5+2\,Na\,Cy=Na\,O,\,NO_5+Na\,Cy,\,Ag\,Cy$. 17 grains of nitrate of silver, or 100 measures of the volumetric solution, representing 5·4 grains of absolute hydrocyanic acid.

"The dilute acid, when pure, is not coloured by sulphuretted hydrogen or precipitated by chloride of barium, showing the absence of metallic taint or sulphuric acid, and no red colour is produced on the addition of the iodo-cyanide of potassium and mercury, showing the absence of any foreign acid.

"The acid known under the name of Scheele's prussic acid contains 4 per cent. of anhydrous acid.

"The anhydrous acid is colourless with a more intense odour than the dilute, sp. gr. 0·697, very volatile, and rapidly decomposed into a carbonaceous-looking matter. The dilute acid can be much longer preserved when a little mineral acid is present, as a trace of sulphuric or hydrochloric acid.

"*Therapeutics.* Anhydrous prussic acid is one of the most intense and rapid of poisons, acting as a direct sedative, apparently from arresting the functions of the whole body; its effects are the same whether taken into the stomach or applied to other mucous membranes, as to the eye, or inhaled as vapour.

"When much diluted, and in medicinal doses, it allays pain and spasm, and if the dose be large, induces giddiness, &c. It is given in painful affections of the stomach and intestines, as in gastrodynia, enterodynia, pyrosis, and vomiting; also in chest affections, as pertussis, asthma, and other cases where the character of the cough is nervous; occasionally it is used to allay palpitation of the heart, especially when connected with dyspepsia, and it has been prescribed in epilepsy, chorea, and other diseases of the nervous system.

"Externally applied it allays irritation of the skin, and when freely diluted may be used in the form of lotion in cutaneous affections accompanied with much itching: great care should be taken that the skin is not abraded.

"*Dose.* Of the acidum hydrocianum dilutum 2 min. to 10 min. Scheele's acid is twice the strength, and it is very desirable that it should not be employed in medicine. Aqua Lauro-Cerasi, or cherry-laurel water, which owes its activity to hydrocyanic acid, is described under Lauro-Cerasus.

"Externally, in the form of lotion, 1 fl. drm. or more may be added to 10 oz. of water, lead lotion, or almond emulsion.

"*Incompatibles.* It is often prescribed with alkalies, as liquor potassæ, &c.; then a cyanide of the metal is formed, which acts in the same manner as the acid; but if a salt of iron be also present, yellow prussiate or ferrocyanide of potassium is produced—a salt possessing none of the properties of prussic acid."

Considering the short time that elapsed between the publication of the *British Pharmacopœia* and the appearance of the present work, and the haste with which, necessarily, the formulæ have been transferred from one to the other, very few errors have found their way into the latter during the process. We have been able, indeed, to detect *but one*, and that occurs in page 273; the diligent reader can find it without requiring our assistance. We now take leave of Dr. Garrod for the present, and trust that the issue of the

companion work, on the value of medicines in the treatment of disease, will afford us an early opportunity of expressing with regard thereto an opinion not less favourable than that which we have formed of the present volume.

An attempt to furnish anything like an analysis of Parrish's very valuable and elaborate *Treatise on Practical Pharmacy* would require considerably more space than we have at our disposal. This, however, is not so much a matter of regret, inasmuch as it would be difficult to think of any point, however minute and apparently trivial, connected with the manipulation of pharmaceutic substances or appliances, which has not been clearly and carefully discussed in this volume. It appears to have been the design of the author to have introduced all the formulæ of the *British Pharmacopœia* into this work; but "the long delay in the revision, consequent on the disagreement on the vexed question of weights and measures, has prevented this, and somewhat limited the sphere of the present edition." The "officinal" formulæ, then, throughout this work are those of the United States' *Pharmacopœia* only; but, in addition to these, a number of very useful and excellent unofficinal formulæ, proprietary medicines, &c., are introduced. In the fifth part, which is devoted to the consideration of extemporaneous pharmacy, are some admirable prefatory remarks on the theory of prescribing, the language used, and the method to be observed in inditing prescriptions. In the section on the mechanical arrangement and the grammatical construction of prescriptions, the student will find some hints that are worthy of his attention. For example:—"The abbreviations allowable in prescriptions might fill some pages if tabulated, but no practical advantage would result from it, while the habit once acquired of *writing every word so fully as that it could be mistaken for no other* would quite obviate the evils complained of." This suggestion is so completely in accordance with what we have already quoted from another source that we are glad of an opportunity of reiterating our approval of the sentiment. Want of space prevents our enlarging further on this valuable work, and we must conclude by a simple expression of our hearty appreciation of its merits.

Contributions to Practical Medicine. By JAMES BEGBIE, M.D., F.R.S.E.; Fellow and late President of the Royal College of Physicians; Physician in Ordinary to the Queen in Scotland, &c.

THE author of the work before us is well known to the profession as a physician of high authority in matters appertaining to practical medicine; and, therefore, his "Contributions" cannot fail to be read with profit, not merely by the young and inexperienced practitioners, but by those who may be looked upon as experienced.

With the exception of the paper on gout all the others have been read before the Medico-Chirurgical Society of Edinburgh, and have been published in the periodicals of the time; but we rejoice to see essays of such great value published in their present form. It is often a subject of regret that some of the most valuable contributions to medicine are scattered amongst periodicals not always within reach, and frequently involving much trouble to the person who is engaged in the search. The practice of reproducing papers, in a collected and more convenient form, is one worthy of imitation when the materials are of the quality which we recognize in these "Contributions" of Dr. Begbie's. The subjects discussed in the "Contributions" are of great interest and importance:—Gout and the Gouty Diathesis; On the Relation of Rheumatism and Chorea; On the Connexion of Erythema Nodosum with the Rheumatic Diathesis; On Anæmia and its Consequences; Enlargement of the Thyroid Gland and Eye-balls; On Dyspepsia and Nervous Disorder in Connexion with the Oxalic Diathesis; On Fatty Degeneration of the Heart; On Erysipelas; On Diphtheria and its Sequels; On the Physiological and Therapeutical Effects of Arsenic; On the Sedative Powers of Datura Stramonium.

The Essay on Gout, the first in order, contains much that is valuable, particularly as regards the gouty diathesis. Many physicians, it has been observed, fail to recognize gout except when it appears in its open and undisguised manner—when it attacks the foot or, perhaps, the toe, failing to perceive its influence in the derangements and diseases of many internal organs.

Dr. Begbie truly observes:—"Gout is a blood disease; it may disclose itself in every organ of the body and complicate and involve every disturbance of the system. It may visit every part and every texture from the crown of the head to the sole of the foot, and molest and vitiate every function appertaining to life."

We agree in the opinion of the author, that dyspeptic symptoms invariably precede even the first invasion of gout. The common idea that a fit of gout exercises a salutary influence on the system has led to much mischief—a belief not exclusively held by the laity, but shared by some physicians. Nothing is more important than to remember "principiis obsta." Judicious treatment on the part of the physician, and the exercise of self-denial on the part of the patient, may, at an early period, check the progress of a disease which, if neglected, is sooner or later productive of irremediable evil.

The transmission of a disposition to certain diseases, without the gout, by parents who have themselves been gouty, is a subject of much interest; this fact is exemplified by the offspring of gouty parents suffering from nephritic disease and cutaneous affections which have been found amenable to the treatment which experience tells us has most influence on gout.

The connexion between rheumatism and cardiac inflammation is one of the best established facts in medicine, but it is not by any means generally admitted that there is any tendency to cardiac complication in the course of gout. Dr. Begbie entertains strong views in favour of the frequency of pericarditis and endocarditis being directly traceable to the gouty diathesis. "May not," he asks, "the transient pangs in the cardiac region, which so constantly affect the victims of the gouty habit, originate in the specific inflammation when it is in the act of depositing lymph which is, by slow, but sure degrees, to form the milky patches, and the small adhesions and the puckered valves, &c., &c." In two instances he has observed pericarditis allied with gout by ties as intimate and indubitable as he has ever known it to be connected with acute rheumatism.

Cases are given illustrative of the part gout plays in some of the organic and functional diseases of the uterine system; and on this point the author quotes a communication which he received from Dr. Simpson:—"I have seen several cases of inflammation of the uterus, or rather of the uterine region, of the nature of simple gout, or rheumatic gout, or, at all events, I believed these inflammatory attacks to be of this special pathological character." Dr. Simpson then refers to a case of pelvic cellulitis which he saw with Dr. Begbie, and which came on during an attack of gout in the fingers. In this case the pelvic effusion was absorbed under local antiphlogistic means and the use of colchicum. In one case of uterine gout Dr.

Simpson refers to, the lady was attacked, repeatedly and metastatically, in the uterus.

Sciatica and other painful neural affections in persons with hereditary disposition to gout are found to yield to colchicum and alkalies after the failure of other agents. The brain and spinal marrow are not exempt from the effects of the gouty poison; the fact of its influencing the former has been long recognized, but not so its operation on the latter. Dr. Graves long since endeavoured to prove that gouty inflammation of the nerves and neurilemma may ultimately, by extending, produce structural change in the spinal marrow. Dr. Begbie, without venturing on a conclusion as to the accuracy of Graves' opinion as to its being peripheral, has no hesitation as to the existence of a peculiar form of paralysis of the lower extremities which he believes to be intimately associated with the gouty diathesis.

The natural history of gout, illustrated by individuals and family groups, is fully entered into. It may appear to many that Dr. Begbie has overdrawn the picture, and that he sees gout, or rather the gouty diathesis, in various organic diseases and functional disturbances which have no connexion with an original taint; but we are not of this opinion, for we are convinced of the great frequency of the gouty diathesis, though we cannot go quite as far as Dr. Gairdner, who believes that "the strumous is not more frequent than the gouty habit." We are bound, however, to admit that the experience of many of the highest authority goes far to confirm the belief of Dr. Gairdner. Sir Benjamin Brodie says:—"A large proportion of the people who come to me with what are esteemed to be local diseases are, in reality, suffering from the influence of the gouty poison in the system, though they have nothing which would commonly pass for gout."

Dr. Begbie, in entering on a consideration of the treatment of gout, quotes, with approbation, the wise saying—that gout, in the early stages, is curable, if not through physic, yet through the physician; but he adds, the remark might be extended to include the patient whose co-operation is so essential. We believe no better rules of treatment can be inculcated than those which Dr. Begbie lays down. He says, temperance, strict temperance, in the gratification of every appetite is demanded of every subject of the gouty diathesis. In a fit of the gout confinement to bed, the foot being elevated and enveloped in warm water dressing and oiled silk—a practice which we have seen productive of great alleviation of pain.

Attention must be paid to the functions of the liver and bowels—to the kidneys and skin.

The salts of potash, in full and continued doses, are recommended in conjunction with colchicum; but the latter, if given alone, is often found uncertain, and of doubtful efficacy. The administration of large doses of colchicum is condemned; though likely to bring an attack of gout rapidly to an end, the system is not relieved. The combination Dr. Begbie recommends is twenty drops of the wine of the seed with scruple doses of the acetate or nitrate of potash three times daily. He believes that colchicum acts beneficially by favouring the elimination of uric acid from the system, and that its effect is best produced when it acts silently and slowly, without depressing the nervous system or exciting, in a marked manner, the secreting organs. Colchicum, with depurants, will be found of advantage in affections of the skin connected with the gouty habit, as well as in inflammatory affections of the eye, and of organs essential to life, after the employment of depletion and mercury—not alone in inflammatory affections, but in headache, toothache, lumbago, sciatica, &c., &c.

When the heart is feeble and dilated, Dr. Begbie recommends iron in conjunction with colchicum.

On the Relation of Rheumatism and Chorea is the subject of the second essay; and here we find our author confirming the connexion which we have often observed, and which was long since noticed by Drs. Copland and Bright. Subsequent writers have, by reference to numerous examples, confirmed the accuracy of these observations. We may refer to the memoir of M. Sée, of Paris, and also to the papers of Drs. Kirkes and Bond, and to the clinical lectures of Dr. R. B. Todd.

On the Connexion of Erythema Nodosum with the Rheumatic Diathesis.—This paper will be found to fairly establish the fact which our author enunciates, viz., that the erythema is connected with a marked diathesis, the rheumatic, and that the skin affection is symptomatic of the blood disease. The reasons which he assigns for the conclusion are the following:—

"1st. The skin affection is most prevalent in, if not confined to, the young; and those under thirty, the chief subjects of the rheumatic diathesis and rheumatic fever.

"2nd. It occurs very frequently in females suffering from menstrual derangement, confirming the views of Drs. Todd, Rigby, and Sir Chas.

Locock, as to the intimate connexion of disorder of the uterus with rheumatic affections.

"3rd. It occurs in connexion with disorder of the general health, characterised by palor, cachexia, and defective excretion; and subsequently developed in febrile excitement, pains in the joints and muscles, and the copious deposition of lithates in the urine; a state of matter analogous to what takes place in rheumatism.

"4th. It is associated frequently with rheumatic fever, co-existing or alternating with it. This is often complicated with the internal disorders with which rheumatism is allied—particularly with pleurisy and pneumonia. It has also been noticed in connexion with endocarditis, or other cardiac affection.

"Lastly. It is successfully treated by colchicum and the alkaline salts, and yields also to a plan of treatment—I mean the use of bark—which was long extensively and successfully employed in the treatment of rheumatism, and still has its advocates among the best informed physicians of the day."

The views of Dr. Begbie, on this subject, have been advocated by many physicians in this country and on the Continent; and may now be considered as accepted facts.

On "Anæmia and its Consequences, Enlargement of the Thyroid Gland and Eye-balls," is a memoir which deserves a careful perusal albeit we cannot altogether agree in the author's views as to the nature of this most interesting affection. No where has the disease attracted more attention than in Dublin, for proof of which we may point to the pages of this Journal, where may be found important contributions to the literature of the subject. If recent writers were so well informed as to the history of this disease, as Dr. Begbie is, we should not find them claiming exclusive credit for one who would himself be surprised if he could know of his posthumous fame.

It appears that Dr. Begbie, in ignorance of Dr. Parry's observations, and of the cases reported by Marsh and Graves, had observed some examples of the triple disease—the cardiac affection, with enlargement of the thyroid gland and protrusion of the eye-balls—a disease which was new to him, and which he believed to be undescribed.

He has now collected together all that is known on the subject, and he has traced its history from the earliest notice of the disease down to the latest observations.

We find that Flajani noticed the enlargement of the thyroid

gland, and the disorder of the heart's action, but Dr. Parry was the *first* who observed the assemblage of symptoms which characterize the disease. His first case dates back to 1786. To Parry, then, belongs the credit of the discovery. For a long period no attention seems to have been directed to the subject, that is to say the triple affection, although the protrusion of the eyes had been noticed by Demours, in 1821. Dr. Begbie refers to Dr. Graves' clinical lecture in 1835, in which he describes the disease "in ignorance of Dr. Parry's and subsequent observations." Soon after Sir Henry Marsh communicated cases to the Pathological Society, and was the first who recorded the morbid appearances observed after death in this remarkable disease. In one of Dr. Begbie's cases, that of a male, he says:—"It is more particularly interesting as affording an opportunity of examining the morbid appearances after death, the only accounts of which that have as yet appeared being those communicated to the Pathological Society of Dublin by Sir Henry Marsh and by Basedow in Germany."

The German author, Basedow, proposed a new name (glotzaugencachexie), and from his having accurately described the disease it was suggested to honour him by associating his name with it; but our author observes, "The still earlier description by Parry, Graves, Marsh, Stokes, and others, renders this inadmissible."

Dr. Begbie next refers to the writings of Schoch, von Graefe, Trousseau, and to the memoir of Withusen, "translated by an accomplished Physician of Dublin, Dr. W. D. Moore;" and having borne testimony to the value of the important contributions of Marsh, Graves, and Stokes, he says:—"In Ireland there have more recently appeared several valuable observations, particularly from Dr. Banks, and the late lamented Dr. Bellingham."

Referring to the theory which explains the condition of the eye by congestion and increased secretion of the humours—a true hydrophthalmia—a theory which he originally held, and a belief in which he shared with other writers on the subject, viz., Mackenzie, Stokes, &c., he now feels constrained by the weight of authority to abandon, and he adopts that of Mr. Haynes Walton, which accounts for the protrusion of the eye by the congested state of the deep-seated veins of the orbit. In only one of the recorded cases has any serious impairment of vision been noted; in all the cases we have ourselves observed the vision was unaffected.

Dr. Begbie, as we observed, on investigating the subject found that the exophthalmic cachexia was described before him, but he has

arrived at conclusions relative to its etiology which had not been previously advanced. They are as follows:—

"1st. That the essence of the disease consists in a vitiated or impoverished condition of the blood—a true anemia.

"2nd. That this abnormal condition, this attenuated quality of the blood, acting directly on the cardiac nerves, through the rapidity with which it circulates, excites the heart and vessels to over action.

"3rd. That the anemic palpitation thus produced is, after a longer or shorter time, followed by turgescence of the vessels of the head and neck, and by enlargement of the thyroid gland and protrusion of the eye-balls.

"4th. That these phenomena are subject to remarkable variations as to intensity, according to the state of the heart's action.

"5th. That the acoustic signs in the heart, in the vessels of the neck, and and in the thyroid gland, are those of anemia, and cannot be referred to any other morbid condition with which we are acquainted.

"6th. That the circumstances of the disturbances of the circulation, the turgescence of the thyroid gland, and the protrusion of the eye-balls, are all capable of mitigation and removal, by means calculated to improve the quality of the blood.

"7th. That the condition of the vascular system, if not removed, issues in organic change, in hypertrophy, dilatation of the chambers of the heart, in enlargement of the great vessels, in induration and structural degeneration of the thyroid gland, and in disorders of vision, and partial or complete loss of sight."

We agree with Dr. Begbie in some of the conclusions he has arrived at, and from others we dissent. We know from observation that the disease may exist without organic cardiac lesion, and we have long considered that this affection, so far as the heart is concerned, may be associated with mere functional derangement, or with profound organic change. In some of the cases recorded by Sir Henry Marsh there was no abnormal sound, and he had no reason to believe that there was any organic disease of the heart. In such cases he found, as we have also experienced, much benefit from a combination of feruginous and sedative remedies. That anemia exists in a considerable number of the cases we are free to admit; in point of fact Dr. Begbie states that he has seen no instance of the disease in which the general and physical signs of anemia were not more or less completely developed. Fully aware of the influences which depraved blood exercises in the production of disease of various forms, we fail at arriving at the conclusion that spanemia alone is sufficient to account for the phenomena. There is

something more in those cases which are associated with spanemia than the signs with which the physician is familiar, as indicative of the latter condition; moreover, we can corroborate the statement of Withusen and others that the disease presents itself in persons who so far from being anemic were plethoric, and abounded in good red blood.

We are then to seek for something beyond mere anemia as constituting the "fons et origo" of these obscure affections.

On Dyspepsia and Nervous Disorders in connexion with the Oxalic Diathesis, is the next essay of Dr. Begbie's series. He depicts with much truth and power the evils which flow from the oxalic diathesis, having pointed out the signs by which it may be readily recognized. The persons who belong to this class suffer from dyspepsia, which may be either of a mild or a most severe character, but often the symptoms are not confined to the digestive organs, but implicate the nervous system, even imperilling the mental faculties. The importance of early detecting the presence of the diathesis, and resorting to appropriate treatment is dwelt upon. Cases are given which illustrate the usual course of the disease and the effect of well-directed remedial measures. The essay thus concludes:—

"The truth of the ancient adage, that the knowledge of a disease is half its cure, is more than confirmed in the diagnosis of the oxalic diathesis; for as certainly as we discover the persistent presence of the characteristic crystals in the urine so certainly do we possess the means of cure in appropriate diet and suitable remedies, unless where long neglect of functional derangement has led to incurable organic disease." He points to the cases he has detailed in confirmation of his statement, and as "affording encouragement to all who are subjected to the vexations and disappointment inseparably connected with the management of the chronic dyspeptic and hypochondriac."

The Memoir on Fatty Degeneration of the Heart is particularly interesting from the fact that it contains the history of the last illness of a distinguished member of our profession who has himself done so much to extend the knowledge of disease.

Dr. Begbie tells us that three years before his death, Dr. Abercrombie had a sudden seizure, with loss of power and impaired sensation over the left side, but without the loss of consciousness, or any affection of speech. The medical man who was called in found him complaining of precordial uneasiness, with slight headache, frequent sighing, cold skin, and pallid countenance.

On this occasion blood was taken both from the temple and arm to a large extent, and active purging resorted to. These active and most unsuitable measures were adopted, Dr. Begbie tells us, by the patients own orders, but it did not occur to any of his advisers to connect the symptoms with the state of the heart. The subsequent feebleness of the circulation might be accounted for by the severity of the treatment which he had subjected himself to. From this attack Dr. Abercrombie was supposed to have quite recovered, but Dr. Alison remarked that he was breathless on ascending stairs a few days before his death, which took place at the expiration of three years from the date of his seizure. In the interval he had no threatening of similar symptoms, and his mental powers were even as vigorous as at any period of his life. His death was very sudden; he had breakfasted with appetite, and appeared to be in his usual health Soon after he was found dead in the water-closet. An examination of the body, which was conducted by Mr. Goodsir, revealed the cause of death to have been fatty degeneration of the heart. It may be that good diet, and a liberal allowance of wine, instead of the "spare diet," might have added years to a life which was so valuable.

Erysipelas.—In the observations on erysipelas, Dr. Begbie strongly recommends the muriated tincture of iron, which was introduced by Mr. Bell. In addition to the cases which he relates, he tells us he has employed it on several other occasions with marked benefit and success. In some cases it has been administered with the most satisfactory results, when the swelling of the face was great, the fever high, the tongue loaded, with restless anxiety and severe headache, and tendency to somnolence. In scarlatina Dr. Begbie tells us it may be administered when the skin is hot and dry, pulse rapid, and urine scanty and bloody, its effect appearing to be to lower the pulse, to relax the skin, and to promote the secretion of urine; and our author remarks that as iron has proved so useful in erysipelas itself, it may not be found unavailing as a resource in those puerperal disorders, hitherto irremediable, with which that disease is in some mysterious manner so intimately associated.

From having for some time employed muriate tincture of iron very extensively in erysipelas and scarlatina, we can add our testimony in its favour to that of Dr. Begbie.

On Diphtheria and its Sequels.—In Dr. Begbie's notice he gives us his impressions of the disease as it fell under his own observation.

He first observed the disease in 1826, and the cases which he has lately observed differed in no essential character from those which he had formerly seen. The conclusions of the author as to the disease are thus summed up:—

"1st. That it is a constitutional disease having the character of fever, running a definite course, and bearing a closer affinity to scarlatina and typhoid fever than to any other specific disease.

"2nd. That its local manifestation is chiefly observed on the mucous membrane of the mouth and throat; the tonsils, uvula, and palate, with the pharynx, being first affected, but that it has a tendency to spread to the adjoining passages, and is particularly prone to invade the larynx.

"3rd. That this local disease is of the nature of inflammation of asthenic character, with exudation of lymph in the form of pellicle.

"4th. That the disease is contagious, and that youth and consanguinity powerfully predispose to it.

"5th. That it is fatal from the severity of the general disease, or from the exudative inflammation invading the larynx, and causing suffocation, or that death may result from the nervous disorder supervening in the form of paralysis.

"And lastly, as we have no specific remedy for diphtheria, the disease and its sequels must be treated on the general principles which regulate our practice in fever, and in inflammation, and in nervous disorders of asthenic character."

Arsenic, its Physiological and Therapeutical Effects.—Dr. Begbie in a former communication, has spoken in high terms of the therapeutic powers of arsenic, and after the lapse of years the favourable judgment he has pronounced has been fully borne out. That it is an agent powerful in some of the most intractable diseases will be admitted by all who have had much experience in its employment.

Dr. Begbie's object is not to enter on the history of arsenic in all its applications to disease, but to confine himself to practical observations on its internal administration in medicinal doses, so as to illustrate the properties of the drug as an ante-periodic and febrifuge, a tonic and powerful alterative. The preparations he has generally employed are Fowler's solution and the arsenious acid; the latter he prefers in affections of the bowels and in uterine disease.

Dr. Begbie cautions us against abandoning the use of arsenic on the first manifestation of its exercising its physiological action on the system—a practice which we know is very prevalent. The medicine should be administered at longer intervals, or in diminished

doses, and thus its curative operation may be secured. The effects of arsenic, in moderate doses (five drops of Fowler's solution), differ as to the period when they show themselves. Dr. Begbie has observed them, generally, in eight or ten days, which is rather earlier than we have noticed the physiological action. With some of the effects produced by the continued use of arsenic most medical men are familiar, but there is one to which Dr. Begbie has been the first to direct attention, viz., a peculiar condition of the tongue, which closely resembles the silvery coating produced by the application of a solution of nitrate of silver.

A copious secretion of urine, often with an abundant deposit of lithates, is observed, in some cases, to accompany the earliest signs of the operation of the mineral. In alluding to the minute papular rash with fine scales occasionally found on the skin, and which causes the surface to assume a dingy brownish hue, he states that he cannot verify the observations of Mr. Hunt, namely, that when the dose of arsenic is diminished, on the occurrence of conjunctivitis, the eye-lids may be allowed to get well; but if the brown state of the skin be kept up the disease will disappear just as rapidly as if the action on the conjunctiva be maintained.

In remarking upon the extraordinary power of arsenic in improving the general health, and imparting vigour to the frame, he says:—The somewhat fabulous experience of the Styrian peasant may be realized; and the increased power of limb, noticed by Trousseau and others, may be actually acquired. The contraction of the extensor muscles of the legs, noticed by Valleix as an effect of the continual use of arsenic, has not been observed by the author. The production of the physiological phenomena of arsenic are not always necessary to insure its therapeutic action; but Dr. Begbie has never seen the agent exhibiting its curative power over severe forms of disease unless when the eye and tongue had presented, and that for weeks together, the proofs that the system had been brought under its influence. Dr. Begbie warns us that the use of arsenic requires much caution; there are persons who cannot bear it, even in small doses—nausea and nervous depression being the inevitable consequences of its administration. The presence of fever, plethora, and an irritable state of the gastro-intestinal membrane, are not, in Dr. Begbie's opinion, contra-indications to the use of arsenic. From the intimate sympathy which exists between the skin and the mucous membrane we might reasonably expect that remedial agents which have a specific action on the skin, would be likely to have a similar

action on the mucous membrane, and experience proves that such is the case. Dr. Simpson, of Edinburgh, has found that in affections of the bowels, in which a process of desquamation takes place, the treatment most efficacious is that which is known to be effectual in diseases of the skin—the arsenite of potash (sixtieth of a grain three or four times a-day). The failure of arsenic, in some cases of disease in which it is administered, Dr. Begbie considers, is some times due to a taint of syphilis, or mercury, or gout, or some other poison in the system. Supplemented by mercury, or quinine, or iodine, or colchicum the wished-for end has been attained.

An aggravation of the eruption is often observed cotemporaneously with the first sign of the physiological action of arsenic; and so far from inducing the physician to give up the remedy should encourage him to persist in its employment. In various forms of uterine disease the obstetric physician finds arsenic a remedy of great value. M. Boudin, Physician-General of the French Army in Algeria, found that arsenic cured some forms of periodic fevers—particularly when complicated with chronic enlargement of the abdominal viscera—which were not amenable to quinine. In bronchitis and hay fever, especially when associated with skin affections, arsenic has been found useful by Dr. Begbie. In blood diseases particularly arsenic has been found to exercise a powerful influence. Dr. Begbie records many cases of cutaneous affections, of rheumatic disease, of chorea, of periodic headache, of tic-doloureux associated with uterine derangements, &c., &c., which well illustrate the effects of judiciously-conducted treatment by arsenic, and fully confirm the high opinion entertained by many practical physicians of its great value as a therapeutic agent.

We consider that Dr. Begbie has done much good in bringing under the notice of the profession his experience on this subject; and we trust that many who are afraid of arsenic may be induced to try it, not merely in diseases of the skin and cognate affections, but in other disorders in which its character has not hitherto been established as the result of such wide-spread experience.

The last paper in Dr. Begbie's volume is a communication of much interest, "On the Sedative Powers of Datura Stramonium." It is chiefly in the form of inhalation that stramonium has been used; but Dr. Begbie has found, in many cases, the extract of the seeds, in doses of a quarter of a grain, every third hour, increased to half a grain, exhibit active narcotic properties and afford relief when other remedies of the same class had failed. The cases in which it has

been found useful are such as indicate a narcotic combined with an anti-spasmodic action. In the paroxysms of dysmenorrhea, in hysteria, tic-doloureux, tetanus; in the dyspnea of phthisis and emphysema its efficiency has been tested. In cases of neuralgia, which unfortunately are by no means rare, in which quinine, iron, zinc, arsenic, iodine, &c., &c., have failed to alleviate pain, Dr. Begbie tells us stramonium will occasionally be found applicable; and though it may not, and cannot, always cure, it will sometimes afford a wonderful measure of relief.

PART III.
MEDICAL MISCELLANY.

Reports, Retrospects, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.[a]

Dr. Mayne, President.

Acute Necrosis.—Dr. M'Dowel exhibited a specimen of acute necrosis of the humerus taken from the body of a girl of ten years of age, who had been admitted into the Hardwicke Hospital, labouring under a high degree of fever of three or four days duration. The pain was intense, and the entire limb swollen to a great degree; the skin was red and shining, and vesicles round the elbow joint gave it the appearance of phlegmonous erysipelas. A few days before admission the child had been attacked with violent pain in the region of the shoulder, became delirious, and was continually screaming. On careful examination, fluctuation was detected under the deltoid muscle, an incision through which, down to the bone, gave exit to an immense accumulation of purulent matter, which entirely surrounded the shaft of the bone, and the flow of which was increased by pressure in the axilla. The case then proceeded favourably for some time, the tumefaction subsided, the vesicles disappeared, and the hard tense feeling of the limb vanished; but before the expiration of a week a change occurred; the pulse rose to 140, and a rose-coloured rash made its appearance (very much resembling that of scarlatina) over the entire body, but there was no soreness of the throat, nor any of the redness of the fauces usually seen in scarlatina. The child ceased to complain of pain, a stupor came over her, a low fever, with muttering delirium set in, and she died in a few days; the rash fading shortly before death.

[a] These reports are furnished by Dr. R. W. Smith, Secretary to the Society.

Upon examination the humerus was found deprived of periosteum throughout nearly its entire extent, and surrounded by purulent matter. Its upper epiphysis was detached, but the scapulo-humeral articulation was healthy in all respects. In the elbow joint was found synovitis, with absorption of cartilage. Here and there the humerus was of a bright pink colour; but by far the greater portion of it was of a dull white colour, and utterly devoid of vascularity, presenting a good example of acute osteitis terminating in necrosis.

In cases such as this, where the inflammation extended from one extremity of the bone to the other, from anatomical considerations, it was to be expected that, while the shoulder joint escaped implication, the articulation of the elbow should be involved in the disease, the junction of the lower epiphysis, with the shaft, being below the line of the synovial membrane; such, however, was not the case with the superior epiphysis, the junction of which, with the shaft of the bone was external to the capsular ligament.

With respect to the cutaneous eruption it certainly was not to be considered as that of genuine scarlatina. It was exceedingly like that which occurred in puerperal women, and which was so very fatal, and of which there had been a great number of cases in the hospital; but the majority were unaccompanied by soreness of the throat. In the present case the eruption was not that of genuine scarlatina, but rather an erythematous rash, perhaps of an analogous nature, but not liable, like scarlatina, to be propagated by the vicinity of a diseased person. Dr. M'Dowel alluded to the memoirs published by his father, in the third and fourth volumes of the *Dublin Medical Journal*, on periostitis complicated with synovitis, in which he mentioned the occurrence of vesicles on the affected limbs, and an erythematous rash over the body resembling that noticed in the present case.—*February* 13, 1864.

Croup.—Dr. Mayne laid before the society the respiratory organs of a child, aged one year and a-half, who had been brought by its mother to the Adelaide Hospital, on the morning of the 8th of the present month, having some time previously suffered from a slight febrile disturbance and an eruption on the skin; the child went through the disease quietly (whatever may have been its nature), but as the eruption was declining, began to cough, and soon became hoarse, and on Sunday night was so extremely restless, being kept awake by cough and wheezing, that the mother, now becoming alarmed, brought it to the hospital in the morning. The rash on the skin seemed to have been varicella, some spots being still apparent in the stage of desiccation. The countenance was good, and did not indicate much depression, but the breathing was croupy and hoarse on coughing; the inspiration being long drawn and sonorous. The first question which arose was whether the disease was genuine

croup or diphtherial croup. The fever was scarcely of so inflammatory a type as to warrant the supposition of its being genuine croup; the skin was not intensely hot; the pulse was 110; the tonsils, seen with difficulty, were enlarged and inflamed; but there was no appearance very characteristic of diphtheria. Next day the child was worse, the respiration more croupy, the pulse accelerated, the inspiratory muscles strained to the utmost, and the voice nearly extinct; the fever had assumed a more asthenic type; and on looking into the throat an exudation was seen on one of the tonsils, but rather a muco-purulent secretion than a tough diphtherial exudation.

On the third day (Wednesday) matters were still worse, and the question of tracheotomy was anxiously considered in the evening. The result was a unanimous decision against the operation, and on the following grounds:—Auscultation showed that, although the left lung was tolerably well filled in respiration, the opposite lung was very nearly silent. Hence it was concluded that the disease was not limited to the trachea alone. It is difficult to arrive at a correct diagnosis in the case of an infant; but the defective respiration of one lung and the comparative clearness of the other was manifest to all. The child died during the night.

On slitting up the trachea there was found a copious secretion resembling a mixture of purulent matter and lymph, which extended into the right bronchus. The subjacent mucous membrane exhibited the most intense vascularity. The left bronchus was free from any morbid effusions, but its lining membrane was congested. The right lung was in a state of *splenization;* but there was this peculiarity, that healthy lobules existed in the immediate vicinity of diseased ones, the line of demarcation between them remaining perfectly distinct. The primary and secondary divisions of the right bronchus were filled with the same kind of secretion as that already mentioned as existing in the trachea.

The result of this case tends to confirm the wisdom of the rule of practice so long adopted by the operating surgeons of Dublin, to be very slow in recommending the operation of tracheotomy for the cure of croup. Whether the case be considered as one of the diphtherial type or of the old form of croup, in neither case could it have been performed with any chance of a successful result.—*February* 13, 1864.

Calcareous Deposit Surrounding the Origin of the Aorta.—Dr. Corrigan said the preparation which he was about to lay before the society had some points of interest. It was a case of permanent patency of the aortic valves, in which there was no disease of the valves themselves beyond a slight contraction, so that the disease, as far as the valves were concerned, was of comparatively little consequence—that is, the man might have lived for a great many years with it, as the patient in this

case had. There was, however, a bony deposit which went completely round the aorta above the valves, forming a continuous rim with osseous plates projecting into the vessel. The first point of interest was the existence of a trumpet bruit audible, without the stethoscope, at the distance of a foot and a-half from the man's chest. That was a very remarkable phenomenon.

Many years since a case of the kind first came under his observation; and on the *post mortem* examination of that case there was found a bony rim round the aorta. It was not only larger than this in extent but it formed a perfect tube of bone, so that when cut out it retained its form as an osseous circle.

Another remarkable case of the trumpet bruit was brought forward at this society by Dr. Banks; but in it there was a double trumpet bruit, a distinct brazen sound, similar to that produced by a trumpet, not only an ascending trumpet sound through the aorta, but also, when the blood was returning through the valves into the ventricles there was another observable; so that for every sixty beats of the ventricle there were 120 trumpet sounds. In the present instance there was only a single trumpet sound consequent on the contraction of the ventricle, the returning blood causing no trumpet sound, not having sufficient force and quantity to produce it. They might venture, then, to consider that trumpet sound as an absolute diagnostic sign of bony deposit in the aorta, either of a rim of bone, as in this case, or a projection, or tongue of bone, as in Dr. Banks' case. These sounds seemed to be produced on the same principle as the sounds that are produced in a common harmonium, in which the tongue of steel is thrown into vibration by a current of air, whilst in the case under consideration it was not the air that threw the tongue of bone into motion, but the blood. The trumpet sound is different from the musical *bruit de soufflet* observed in chlorotic females. It is difficult, or indeed impossible, to convey by words the distinction between the sounds; but there is this distinguishing character easily recognized. The trumpet sound is invariably synchronous with the action of the ventricles or the descending current. It occurs only when the blood is in rapid motion, when a bony tube or tongue of bone is kept in vibration by the action of the ascending or descending blood. The musical bruit of chlorotic cases is never heard at a distance from the patient. It is not carried through the circumambient air; whereas in the case before them, it was heard as mentioned at a distance from the patient. It might not, however, even in similar cases, be so loud as to be heard at a very short distance; but there were other signs of distinction. In the musical *bruit de soufflet*, or *bruit de diable*, as it was sometimes called, the motion of blood in the jugular vein in which the sound is produced has no relation to the action of the ventricle. It is a continuous sound, or it swells, decreases, and disappears, but has no synchronism with the

action of the ventricle; and it can always be stopped by pressing the thumb lightly over the jugular vein above the stethoscope, so as not to interfere with the action of the blood in the carotid. He thought they might go so far as to say that the trumpet sound was diagnostic of the deposit of bone, and of bone thrown into vibration.

A question of interest then arose. Did the existence of that sound indicate that life would be more shortened by the disease of patency than where it did not exist? He did not consider it did; for in these and other cases in which the sound had been observed, the persons had lived for a great number of years. The next point of interest in the case was the length of time during which permanent patency might exist and not interfere with the patient's pursuits. In this present case, the man, who had been a lamp-lighter—an occupation requiring a great deal of muscular exertion—was twenty-eight years of age when admitted into hospital. He had had rheumatic fever when he was twenty years old, and they might conclude that the heart disease commenced then, for it was a common result of rheumatic fever. But, notwithstanding the permanent patency he continued his work until four or five days ago, when he was exposed to inclement weather and very hard work during the night. He came into hospital labouring under an attack of pulmonary apoplexy, of which he died, throwing up large quantities of blood. The last and most interesting point is that tolerably active pursuits, a fair amount of nourishment, and abstaining from any treatment calculated to depress the muscular parietes of the ventricles, afforded persons suffering from the disease the greatest prospect of a long life; and lastly, it showed that a man labouring under it might continue to carry on his ordinary avocations without being much troubled by the disease. This man had had the disease for eight years.—*February* 20, 1864.

Ulceration of the Intestines.—Dr. Mayne observed that the specimens to which he was desirous of calling the attention of the society were illustrative of typhoid fever, and of interest, as showing that in its earlier stages the disease was apt to run rather an insidious course—that the symptoms at first might not be very defined or well marked, that danger might be impending all the time, and that finally, when the alarming symptoms set in, the disease might rapidly run a fatal course.

The patient was a man, aged twenty-one, and had generally enjoyed excellent health. On Monday, 8th, while working in a vitriol manufactory, he was suddenly seized with a very severe rigor, followed by headache, vomiting, and diarrhea, and retired early to bed. On Tuesday he did not appear to have any marked symptoms, but was unable to resume his usual work. On Thursday he procured admission into the Adelaide Hospital, and I saw him on Friday, the fifth day of his illness. The usual symptoms of mild fever were present, but there was as yet nothing

very striking in the case. He had been restless, and had not slept during the night. He merely complained of headache; his pulse was about ninety, and the skin not very hot. He had some diarrhea, and the alvine discharges were of an ochreous character, fluid, and without any tinge of blood. He had been coughing a little in the morning, and bronchial râles were heard here and there. The abdomen was slightly swollen and tympanitic, but there was no pain on pressure, nor gurgling. There was no trace of any rash on the skin. On Saturday, the sixth day of his illness, it was reported that he was better; he had slept during the previous night, had less headache, and expressed himself as about to get well; there was less tympany, and the stools were neither so copious nor so frequent as they had been.

On Sunday evening, however, he was attacked with intense pain in the lower part of the abdomen, the seizure having been immediately preceded by a short but severe rigor; he was also suffering from great irritability of the bladder, being constantly desirous of passing water. A catheter was introduced, and a large quantity of urine drawn off, but without affording the patient any relief. Upon the following morning there was a great change in his appearance; he had the sharp, pinched countenance of a man suffering mortal agony; his knees were drawn up, the abdomen tympanitic to a great degree, and altogether intolerant even of gentle pressure; he was vomiting, and had all the appearance of peritonitis, the result of perforation. He died during the night.

Autopsy.—There was no escape of gas when the abdomen was laid open; but a copious stream of serum issued forth, mixed with flakes of lymph; when this had all escaped the most intense peritonitis became visible, the intestines being glued together by large quantities of coagulable lymph. The anterior surface of the intestines presented numerous discoloured patches, some of a pink colour, others of a purple hue; and in some of the latter the intestines seemed thin and as if about to give way; but no perforation could be discovered. The duodenum and jejunum were comparatively healthy; but the ileum presented the well-marked ulcers generally present in typhoid fever. In the upper portion of the intestine they were few in number and of small size, some of them bearing a strong resemblance to a chancre. Proceeding downwards they became more numerous and of larger size. It was remarkable, that, although the disease ran its course in eight or nine days, the ulcers had gone through all their stages, sloughs having been detached from some of them; but no where had even the most minute perforation taken place, but in the situation of some of the ulcers the intestine was like a piece of silver paper, and quite transparent. The mesenteric glands connected with the small intestines were very much enlarged.

Dr. Mayne observed, in conclusion, that on looking into the works of various authors he did not find any case recorded in which fatal

peritonitis had set in so suddenly as in the present instance, independent of perforation. However, it was not difficult to understand how, in cases where the ulcer had penetrated so deeply as to lay bare the peritoneal tissue of the intestines, the inflammation might be propagated to a contiguous surface so as to set up a fatal peritonitis.—*February* 20, 1864.

Cancer of the Liver.—Dr. Stokes, at the request of Mr. O'Ferrall, exhibited a specimen of cancerous tubercles of the liver, taken from the body of a female, aged sixty-one, who had been lately admitted into St. Vincent's Hospital. She was deeply jaundiced, had a stupid expression of countenance, and a great inclination to sleep. The liver was enlarged, hard, and tuberculated, and extended across the left hypochondriac region. She had also ascites. The tendency to coma gradually increased until her death, which occurred just five weeks after her admission.

The *post mortem* appearances were the following:—The liver contained numerous cancerous tubercles (Farre's); and a section of it showed great biliary congestion; the branches of the hepatic duct were compressed by the tubercles. There were numerous enlarged glands along the concave margin of the stomach. The gall-bladder was distended with dark viscid bile, but there were no calculi. The ductus communis was nearly half an inch in diameter, but close to its entrance into the duodenum it was contracted and surrounded by enlarged muciparous glands. The cystic duct was unusually large.

Dr. Stokes observed that the case illustrated two of the many causes of jaundice, namely, obstruction by pressure of tubercles on certain branches of the hepatic duct, causing retention of bile in that portion of the liver to which they belonged; and secondly, obstruction of the common duct owing to contraction of this tube at its entrance into the duodenum, which would not admit of the passage of a probe. The presence of Farre's tubercles in this case (as in many others formerly brought before the society) did not appear to be the cause of the retention of the bile, except at the points where individual tubercles compressed certain branches of the hepatic ducts.

The morbid deposit surrounding the orifice of the ductus communis reminded him of a case of jaundice occurring in a gentleman who had been some time ago under his care. He had from between the commencement of his illness and his decease (a period of about a year and a-half) nearly thirty attacks of the following nature:—First, his skin became intolerably itchy; then followed a very severe rigor, succeeded by a hot stage, but no sweating, and then complete and universal jaundice; and as soon as this appeared, the patient recovered his feeling of comfort and was able to resume his ordinary avocations. Then the

jaundice would gradually disappear, and another attack of a similar nature would come on after a short time.

Upon examination after death the liver was found to be quite healthy; but a circle of cancerous deposit surrounded and compressed the orifice of the common biliary duct. A very careful examination failed to detect any other diseased condition.

In the case before the society there had not been pain; and, it was not unimportant to remark, that in a large number of visceral cancers (especially those of the lung and liver), there was either an absence of pain, or, if present, it was so slight as to have nothing characteristic about it. So also with respect to jaundice; it was well known that a very extensive deposition of cancer might take place in the liver without the occurrence of jaundice, and it was probable that, when it did occur, it was to be attributed to a mechanical cause. In Mr. O'Ferrall's case the enlargement of the liver had a double mechanical origin, namely, the extensive deposit of cancerous tubercles and the distention of the biliary ducts, a cause of enlargement of this viscus first observed by Mr. Stanley of London.

In illustration of this he might mention the case of a woman who had been not long since brought into the Meath Hospital suffering from jaundice which was unaccompanied by fever, pain, or hepatic enlargement. It appeared at first to be an ordinary case of jaundice from duodenal irritation; the woman remained in this condition for several weeks, but a few days before her death the liver rapidly tumefied. It was found, upon examination, that the common duct had become completely obstructed by large biliary calculi, and the divisions of the hepatic ducts were dilated into pouches, from which, when punctured, bile spouted out.—*February* 20, 1864.

Gammarus Pulex.—Dr. Banon exhibited several specimens of the gammarus pulex, or fresh-water shrimp, which had passed from the bowels of a female lunatic, who had been in a partially paralysed state for several months. After having taken a draught of castor oil and turpentine, she voided the specimens in question; the nurse found them in the discharges upon two occasions, upon each of which the occurrence followed the administration of turpentine. They were, when voided, alive and active. They are very common in pipe water, are carnivorous, feeding principally on dead fish, and often on each other. According to Professor Zenker, they undergo no metamorphosis, and are capable of being kept alive in water, even when not exposed to the air. It might be a question whether they had been taken into the system in the grown state or whether their ova or larvæ were generated in the body. He put some of them into water containing a few drops of muriatic acid and they died at once. This would lead to the supposition that the gastric

juice in the stomach would prove fatal to them, and that the ova were generated in the system, but that it is known that the gastric juice will not act in the stomach in destroying living creatures or tissues, as weak mineral acid mixtures will outside the organ.

There were several remarkable cases on record in which insects of various kinds were passed from the bowels. Dr. Pickels recorded one in the fifth volume of the Transactions of the Provincial Association, in which the patient passed several thousand insects, particularly of the beetle tribe, continuing to do so for several months. In that instance a number of the larvæ were passed. Dr. Banon, in conclusion, stated that Mr. M'Alister, to whom he was indebted for much information on the subject, had shown him numerous specimens of the gammarus pulex found in water taken from the fountain erected lately to the memory of Sir Philip Crampton.—*February* 27, 1864.

Ascaris Lumbricoides.—Dr. Law presented a specimen of ascaris lumbricoides, about a foot in length, and apparently full of ova, passed by the mouth by a female, aged twenty-two, while labouring under typhus fever. She had been in attendance on her mother who died of fever in the Isle of Man. She had a bad passage to Liverpool upon leaving the Isle of Man, and an equally severe one from Liverpool to Dublin. On landing she was brought to Sir Patrick Dun's Hospital, labouring under typhus fever. Her prostration was such as to require an unusually large amount of stimulants. The most remarkable feature in her case was the subsultus tendinum which was so severe as almost to prevent the pulse being felt.

There was but little change in the symptoms until the seventeenth day, when diarrhea set in, and she fell into a deep stupor. In the evening of that day she had a violent attack of coughing, and the nurse, supposing that she was choking from a collection of mucus in the mouth and throat, came to her relief, and drew out of her mouth the worm now exhibited. Upon the following day she was visibly improved; she seemed as if she had awoke out of a deep sleep. It appeared as if she had had a crisis—at least what would formerly have been regarded as such. There was certainly something like a *perturbatio critica*, an aggravation of the symptoms, and then the patient began to amend. In similar cases of typhus fever, recently under his care, there had been undoubted evidence of crisis, although less distinct than he had formerly seen. In reference to the share the expulsion of the worm had in the improvement, he should only say that the subsultus tendinum ceased when it was removed. This symptom might fairly be attributed to the irritation of the lumbricus, just as the presence of worms in the intestines of children produce convulsions. As to how much of the improvement was to be ascribed to the operation of a blister which he

had directed to be applied to the vertex, he had too much experience of its value, when thus used, in recovering from coma and stupor in fever, not to allow it its share in the result. The woman had a rapid convalescence.

Dr. Law observed that no subject had perplexed the physiologist, the pathologist, and the naturalist, more than that of entozoa, as to their origin, their propagation, and ultimate destination. From their being destitute of reproductive organs, and their being found in situations where ova could not have been introduced *ab extra*, no other theory than that of equivocal generation seemed competent to explain their origin. Von Siebold, of Munich, from observations and experiments, discovered in the habits of these animals certain propensities which explained much of what before seemed embarrassing. He ascribed to them migrating, emigrating, and immigrating propensities; the first showing itself in their moving from place to place in the same individual, and thus often making their way into very inconvenient situations. The late Dr. Power had exhibited before the society a large number of lumbrici in the hepatic ducts; and Dr. Law had shown one that escaped through an ulcer in the groin where an ileo-cœcal abscess had opened. Andral has recorded a case where one of these entozoa, getting into the trachea, caused death by suffocation. It can easily be imagined that the specimen now exhibited was either *en route* towards, or had already made some way into, the trachea, from which it was expelled by coughing.

With regard to their emigrating propensity, they were observed to leave the animal in which they were produced and to make their way into other animals, where they underwent a further development, and finally attained to maturity and the capacity of reproduction. They seemed to possess an instinct which guided them to the animals suited to their sojourn for this ulterior development. Dr. Law thought that this instinct was shown in their making their way into the higher animal when it was reduced by disease, and while the system remains in this depressed state so long, to use the expression of De Filippi—"*gli ospitanti ed ospitati vivono in perfetta armonia, gli uni non disturbano il negolare procedimento delle favi vitali negli altri.*" The Danish naturalist, Steenstrup, contributed much towards reducing the facts collected on this subject to some degree of order by the discovery of the law which he designated, "alternation of generations," and which may thus be described—that an animal bearing young which are, and remain dissimilar to their parent, but bring forth a new generation whose members, either themselves or in their descendants, return to the form of the original parent. The discovery of this law meets many of the most perplexing points connected with these animals. Thus the parent produces an offspring designated a nurse, or agamozooid, which brings forth young without itself possessing any sexual organs. The labours of von Siebold and Steenstrup have

accounted for much that appeared to admit of no explanation, except what a questionable theory, viz., that of equivocal generation, afforded.—*February* 27, 1864.

Cataract.—Professor Haughton exhibited the eyes of a seal that was lately living in the Zoological Gardens. He said that on last Monday fortnight (February 8th), the seal was attacked by some boys with brickbats, and received injuries from them, in consequence of which he died in fifteen days afterwards. The immediate cause of death was starvation. He refused food after receiving the blow, and no inducement could persuade him to eat. A few days after the receipt of the injury the vision of one eye was observed to begin to get impaired, and before he died he was completely blind of the right eye. In that eye cataract was to be seen; the lens was quite opaque, and also somewhat discoloured; there was also a small circle of pink round the margin of the cornea. The blow which killed the animal was on the right side of the mouth, and was so violent that it broke the large canine tooth on that side and seriously injured the mass of nerves that supplied the papillæ of the hairs on the mouth of the animal. The discolouration of the lens was, perhaps, the result of direct violence, and the cataract might be attributed to the inflammation that was set up after the injury.—*February* 27, 1864.

Laceration of the Lungs.—Dr. Robert M'Donnell brought the following case under the notice of the Society:—

A young man, named Patrick Mooney, aged twenty-four, was admitted into Jervis-street Hospital on the evening of the 15th of February; Dr. M'Donnell happening to be at the hospital saw him at the time of his admission, and learned from him the particulars of his case. Two hours before, while loading a cart with manure, the horse started, he was knocked down, and the wheel passed across his body; appearing seriously injured, he was conveyed on a cart a distance of four miles to the hospital. On admission he was not in a state of such extreme collapse as to warrant the supposition that there was an extensive rupture of any internal organ; the pulse, although rapid, was easily counted, the surface warm, countenance anxious, respiration short; he chiefly complained of "his belly" as the seat of injury, and the region of the liver appeared to be the most tender part. There was no evidence of fractured rib, no hemoptysis; he passed water freely, of natural colour; his spine was uninjured. On proceeding to examine his chest, he smiled at the idea, as though he felt there was nothing wrong there, and putting his hand on the epigastrium said, "Oh, my belly, it is my belly." Respiration was audible in both lungs at this time. On the following day his pulse was ninety, and full; he had quite rallied from the condition of collapse; he still complained of much pain over the liver and epigastrium. On

the 18th he became slightly, but decidedly, jaundiced; general condition as before—pulse ninety, urine deeply coloured with bile, but containing neither sugar nor albumen.

On the 20th he became suddenly much worse; his breathing rapid and laboured; pulse 120–130, intermitting; countenance much depressed; sweating profusely, dreadful pain in the epigastrium. On examination the left side of the chest was found to be tympanitic, with respiratory murmur very feeble; the heart displaced to the right side of the sternum, and extensive dulness over the lower portion of the left side of the chest. There had been as yet no hemoptysis, no trace of external emphysema, no audible metallic tinkling or amphoric resonance; neither did the ecchymosis described by Valentin exist in the lumbar region.

On the 21st, in spite of the administration of opium freely, his suffering was extreme; it was hardly possible to examine him so incessant were his cries, "Oh, my poor belly; open it, and let the wind out."

As there was no doubt as to the fact that blood and air were pent up in the cavity of the left pleura, it was determined, in consultation, to relieve the patient by tapping. Just before proceeding to perform this operation, metallic tinkling and amphoric resonance were heard for the first time, leaving no doubt as to the existence of a wound of the lung. The trochar was introduced two inches external to the nipple; twenty-two and a half ounces of fluid blood (which afterwards coagulated firmly), were drawn off, besides a large quantity of air; the trochar was retained in its place guarded by a valve of membrane to prevent the ingress of air, but permitting the escape of fluid, which dribbled away freely all day. The relief to the patient was complete; his pulse ceased to intermit, he slept soundly. He became, however, gradually weaker, and died, the right lung being attacked by bronchitis, on the tenth day from the receipt of the injury.

Dr. M'Donnell observed that the *post mortem* examination, which was made in presence of the pupils of the hospital, was executed with a great deal of care, raising the sternum and removing it without opening the left pleural cavity. They were thus enabled to see the lie of all the parts. The heart was displaced and pushed over to the right side; the stomach displaced downwards, and the diaphragm so pushed down as to be quite thrown below the cartilage of the ribs. It was remarkable that the heart's sounds were distinctly audible under the left clavicle, notwithstanding its great displacement. The existence of the sounds in that region made one almost timid about making an opening in the front part of the chest. On raising the sternum the mediastinum was found to be pushed over, and a considerable quantity of gas occupied the left pleural cavity, on opening which they found it to contain at least three quarts of blood. The lung was lacerated in a terrible way—a large portion of it being nearly torn off, but no rib was broken; and they

therefore supposed that the extensive laceration of the lung had resulted from concussion from the simple shock. Some of the blood on the upper surface of the diaphragm and inside of the pleura had coagulated, but the greater part of it was quite fluid. It appeared as if a part of the lung had been adherent when rent off by the shock. The lung was collapsed and lying against the spine. About an hour or two before his death some clots of blood were spat up; but until the latter moments of his life no hemoptysis occurred. Such cases, he believed, were not very common. It was the second case of the kind he had seen.

Dr. Lyons reminded him of a case that he met with in the Crimea, in which a person wounded by a round shot had his lung greatly lacerated without any fracture of the ribs. And Professor Smith told him that he had seen four such cases—one being that of a dog run over in the streets of Dublin, in which the lungs had been lacerated to a considerable extent, without fracture of any rib. One of the most remarkable symptoms in the case under notice was that the symptoms of pneumothorax and hemorrhage did not supervene until the 5th day, so that he was forced to suppose that even in the extensive laceration which took place, that the lung did not bleed at the time. The man could not, at the time of his admission into hospital, have lost a large quantity of blood into the pleural cavity. It was somewhat analogous to cases where the wound was occasioned by a round shot or jagged instrument. The collapse was not such as to warrant the suspicion of rupture of an internal organ. He was led to suppose that the laceration so tore the vessels and tubes that they did not let out the blood and air in the quantities that escaped some days after the injury had been received.—*February* 27, 1864.

Erysipelas.—Dr. M'Donnell exhibited a limb, removed from a man twenty-eight years of age, who was admitted into Jervis-street Hospital, suffering from a severe attack of gangrenous erysipelas. He said that he had met with no injury, but on the side of the great toe there was a slight abrasion. The foot was greatly swollen. There were large vesicles filled with a dark-coloured fluid, which, being let out, a large gangrenous patch could be seen underneath. The constitutional disturbance was very great. The inflammation rapidly extended up the limb, and phlebitis set in along the course of the saphena vein. The pulse was rapid, and exceedingly weak, and the man was altogether in the depressed condition of a person suffering from a severe injury—so much so, that measures were taken to resuscitate him from almost a state of collapse. We visited him again on the same day, and made four incisions upon the dorsum of the foot, from which there trickled out matter which was infiltrated in the cellular tissue. After several days the erysipelas spread, but did not go very far above the knee. The integuments of the calf of the leg sloughed,

leaving extensive ulcers. The irritative fever was intense; the pulse was 130 in the morning, and 140 in the evening. He continued, however, all through to take nourishment freely. On the 1st of March the limb was removed, and the man was still going on very well. The case was worthy of the notice of the Society, inasmuch as these terrible instances of gangrenous erysipelas were fortunately not of very frequent occurrence in civil practice.—*March* 5, 1864.

Ulcer of the Stomach.—Dr. Beatty brought under the notice of the Society a specimen of perforating ulcer of the stomach, taken from the body of a female who had been under his observation on three different occasions. Upon the first of these she walked into his study, complaining of pain in the region of the stomach, but looking well. She was about twenty-five years of age. The pain was not, at this period, severe, but in accordance with her wishes she was admitted into the City of Dublin Hospital. At the end of a week she thought herself so much better that she left the hospital, and he saw nothing more of her until last Monday, when she again came to his house, complaining of a similar pain. She still appeared in good condition, had no vomiting, but complained of a burning kind of pain, extending from the stomach downwards, over the rest of the abdomen.

On last Wednesday night he saw her at 11 o'clock, when he found that at 6 o'clock, p.m., the same day, she was suddenly attacked with violent pain in the abdomen, causing her to scream. From that time the pain continued with great intensity, but without any vomiting. He found her lying on her back, with her knees drawn up; she was covered with perspiration; the pulse was 130, and signs of collapse were apparent. Twelve hours afterwards she began to vomit, and rejected anything that entered the stomach, and died twenty-three hours from the setting in of the severe pain. The diagnosis arrived at was—perforation of some portion of the digestive tube.

Autopsy.—A perforation as large as a shilling was found in the anterior wall of the stomach. It was circular, well-defined, and with a thick elevated margin. This portion of the stomach had, however, become closely adherent to the under surface of the left lobe of the liver, so that there was no escape of the contents of the former organ. There did not appear in this case to be any cancerous disease, but there was some thickening of the tissues of the stomach.—*March* 5, 1864.

Arachnitis.—Dr. Banon exhibited the morbid specimen, and described a case, of cerebral disease in a mare, which had come under his notice within the last few days. On Monday last he was visiting a friend in Merrion-square, who asked him to consult with Mr. Ferguson, veterinary surgeon, who was at the time in attendance on a valuable mare belonging

to the family. From the account given him of the symptoms, and from the circumstance of the animal having had a rigor, Dr. Banon considered it to be a severe febrile attack. On consultation, however, with Mr. Ferguson, to his surprise, that gentleman told him that the disease was acute inflammation of the arachnoid membrane, intervening between the medulla oblongata and the cerebellum, and added that the animal would die within three or four days. The result proved the correctness of the prognosis, for the mare became rapidly worse, and the case so hopeless that, at the owner's request, the carotid arteries were divided.

Dr. Banon read the following account of the case, written by Mr. Ferguson, and containing that gentleman's views respecting the nature and pathology of the disease:—

"Chronic disease of that portion of the arachnoid membrane intervening between the medulla oblongata and the cerebellum, is frequent among horses, particularly female ones; and generally, sooner or later, gives rise to very marked symptoms, which have hitherto been, by veterinary practitioners, mistaken for indications of other affections. When the disease has advanced so far as to interfere by pressure, or by deranging the circulation of the cerebellum, the animal, if a female, evinces intense and continuous desire for sexual intercourse. There is also a disposition to catch hold of objects with the teeth, retain them there, or continue gnawing them. Sometimes the first symptom observed is a rapid snapping of the teeth together, at short intervals, much resembling what occurs in a rigor, though not so prolonged. The pulse is generally slow, the respiration natural, though sighing is often observable. In some cases there is no diminution of appetite—in others there is, but not at first. The uterine irritation increases; the propensity to bite becomes greater; the animal tears everything within its reach—even the wall of the stable and the ground, the paving of which it often tears up partially. At this stage the affection is generally mistaken for hydrophobia. In the beginning it is not unfrequently supposed, from the frequent attempts to pass urine, to be an affection of the kidneys or bladder. Despite of every treatment, such cases terminate fatally. In some the progress is very gradual—in others the patient dies in three or four days.

"A marked case of this affection recently presented itself. An aged brown mare, the property of Mr. K——, of Merrion-square, was observed, on last Monday morning, when she was being harnessed, to appear very uneasy. The servant thought she was getting colic. Having been sent for, I found her very restless, but with a natural pulse and respiration I observed, however, the snapping of the teeth, and the genital excitement to which I have already alluded, as striking symptoms of this disease. Much to the astonishment of the persons present, I pronounced the case as hopeless. Towards evening, Dr. Banon, happening to be at the owner's residence, was requested to ask my opinion as to the probable result. He

was also good enough to look at the animal with me. I told him my reason for making so unfavourable a prognosis, which was, that I considered that the animal had chronic granular disease of the arachnoid membrane, at the base of the cerebellum, of long standing. The mare got rapidly worse, tearing with her teeth—not alone the wood-work of the stable, the walls, and even the pavement, but also herself. She became so violent, that on Thursday, the fourth day of the attack, I was compelled, by the owner's direction, to have her put to death, which was done by dividing the carotid arteries. On the previous evening she had lost nearly four quarts of blood from the jugular vein.

"On making a *post mortem* examination, all the thoracic and abdominal viscera were found healthy. The uterus, ovaries, and vagina, presented merely the appearances usually seen in mares when in a state of œstrum. On examining the brain, the arachnoid membrane, particularly that of the cerebellum, notwithstanding that the animal had been bled to death, was found very much congested. At the junction of the cerebellum with the medulla oblongata there was the granulated mass of diseased arachnoid membrane, such as I had stated would be found. It contained small calcareous deposits, like small shot, and the substance of the cerebellum, in contact with it, was disintegrated."

Dr. Banon, without pledging himself as to the views and deductions drawn by Mr. Ferguson, bore testimony to the correctness of the account given of the symptoms in this particular case, which he had watched with great interest.—*March* 5, 1864.

Tubercles in the Lungs and Peritoneum.—Dr. Banon exhibited a specimen of tubercular disease of the peritoneum, occurring in a phthisical patient, a girl aged twenty-two, who had been committed, in December, 1862, to the Mountjoy female prison. She was admitted into the hospital department last August, labouring under ascites. On the 20th of October she was tapped, and seven gallons and a half of fluid were drawn off. The girl lived until the 22nd of last month, and never had a return of the ascites. She died of pulmonary consumption.

The lungs were found, upon *post mortem* examination, completely filled, throughout their entire extent, with tubercular matter, and both the visceral and parietal layers of the peritoneum were studded thickly with miliary tubercles. The frequency of the deposit of tubercular matter on the mucous surface of the intestinal canal, in cases of phthisis, was remarkable, but in this case the mucous membrane was free from any such deposition. The liver was in a state of fatty degeneration. The other viscera of the thorax and abdomen were healthy.—*March* 5, 1864.

Paronychia.—Mr. Tyrrell exhibited a cast of a man's hand, showing gangrene of the right fore-finger, and also the gangrenous finger itself.

The patient was admitted into Jervis-street Hospital on the 20th of February, and the account he gave was—that eight days previously he was out fishing, and he was clearing out his net and taking a Medusa from it, when he found a sudden stinging sensation running up his finger (which had been slightly wounded before), as far as the metacarpal bone. The pain gradually subsided, and he took no more notice of the matter for two days, when, swelling and pain in the finger coming on suddenly, he poulticed it for two days; and, on the fifth day after the sting of the Medusa, he went to a surgeon, who opened the finger, but it must have been mortified at that time, inasmuch as he said the operation did not cause him any pain. On the seventh day he came to Dublin, and was admitted into Jervis-street Hospital. The entire finger was gangrenous, and a considerable amount of inflammation extended upwards along the anterior part of the hand, and likewise the back part of it; and on pressing on the palm, pus exuded, showing that the disease had extended higher up in the subcutaneous than in the cutaneous structures. After the man had been in hospital a few days, and when the line of demarcation was distinctly formed, Dr. Tyrrell removed the finger and a part of the metacarpal bone. He did not like to perform the operation recommended by Mr. Ferguson—of removing the metacarpal bone obliquely, inasmuch as the man, being a sailor, had laid great stress on the breadth of this part of the hand, as enabling him the better to lay hold of ropes, and he preferred having the bone cut off straight. This was a true case of paronychia gangrenosa, different from that form mentioned by Dr. Todd in the second volume of the *Dublin Hospital Reports*, which was of a constitutional nature. The only question was, whether they were to ascribe the rapid death of the part to the sting of the Medusa. It was well known that these jelly fish, when taken hold of, often stung the hand very smartly, and it might have happened that the urticating, or poisonous liquid, got into the slight wound in the man's finger and poisoned the blood, so that when inflammation arose from the wound, it rapidly ran on to gangrene. It was a very rare specimen of the disease, and might, perhaps, be of some importance, as a warning that we should not handle these animals lightly if there should be any wound in the hand.—*March* 12, 1864.

Hematocele.—Dr. Robert M'Donnell brought forward the following case:—A middle-aged man was admitted to the hospital of the Mountjoy Convict Prison on the 26th of July, 1863, having a tumour in the scrotum the size of a child's head. He gave the following history of his case, but without much precision or clearness:—A swelling had first made its appearance between three and four months before, and having attained a size about equal to his fist, was tapped by a surgeon, and a clear straw-coloured fluid drawn off. After an interval of a month the operation

was again performed, seemingly with perfect success. A month before he came under my notice he had, a third time, been tapped. On this occasion, he said, that the quantity of fluid drawn off was less than on the other occasions; a considerable sized tumour remained behind; and, on the day after the tapping, the swelling was as great as before the operation.

The man's aspect was not unhealthy, and he had two grown-up sons, also healthy men, inmates of the prison. The scrotum was of a dusky red colour, with enlarged veins ramifying over the tumour; the glands in both groins were enlarged; the swelling, which was very inconvenient to him from its bulk and weight, had all the external look of a malignant tumour. It was uncomplicated by hernia, was perfectly opaque when examined by light, gave an obscure feeling of fluctuation, and was distinctly resonant—I may say tympanitic—on percussion. At the lower part of the tumour there was a minute opening, from which some drops of turbid serum, having a peculiarly fetid odour, could be expressed. The tumour was regarded by myself, and the surgeons who saw the case in consultation with me, as a hematocele which had undergone decomposition, and thus given rise to the formation of gas in the tunica vaginalis; but the question was, whether malignant disease of the testis co-existed with this condition. The look of the tumour, its great size, its dusky redness, the veins ramifying over it, as well as the enlargement of the glands in the groin, favoured this view. Some, regarding the disease as malignant, and so advanced as to have contaminated the neighbouring glands, doubted the propriety of removing it. Professor Syme of Edinburgh, happening to be in Dublin, saw the case; and as he and my colleague, Dr. Banon, concurred with me in deeming it advisable to remove the tumour, I did so on the 18th of August.

There was nothing peculiar in the operation. The écraseur was used to divide the cord, and from its vessels, which were not afterwards ligatured, not one drop of blood escaped. The tumour here exhibited gives no idea of the bulk of the mass at the time of removal. There was so much gas within the tunica vaginalis, that the whole floated when thrown into a bucket of water. The gas collected proved to be sulphureted hydrogen. The tumour was in no respect malignant in its nature—it was simply a decomposed hematocele, as seen in the accompanying drawing. The walls of the tunica vaginalis are immensely thickened; the blood clot softened and rotten; but the testicle, although flattened and atrophied by pressure, free from disease.

The patient made a rapid recovery, and is now perfectly well.—*March* 12, 1864.

Ulceration of the Intestines.—Dr. Henry Kennedy exhibited a specimen of ulceration of the ileum, taken from the body of a man who had died

of enteric fever; but in whose case the symptoms were so mixed that he thought it worthy of being put on record. On 22nd of May, 1863, the late Dr. Neligan sent the patient into Sir Patrick Dun's Hospital. He was aged twenty-six, and of a highly nervous temperament; and his fever was said to have arisen from fright, owing to a pair of horses he was driving having run away. He struggled against his illness till four days before admission. When first seen by Dr. Kennedy he had been eight days ill; and had the look of a man who was likely to have heavy fever, the brain being the organ then engaged. He had a brilliant injected eye, with trembling and nervous twitchings about the face; the skin was hot. On the eleventh day diarrhea had set in, and for the first time a few spots could be detected on the abdomen; and signs of enteric fever afterwards showed themselves. *These entirely subsided by the fourth day from their onset:* but not so the fever. The spots became darker, much more numerous, and spread over the chest, front of the neck, and down the arms, and well marked petechiæ could be distinguished amongst them. The fever became genuine typhus; the tongue was dry and brown, and sordes collected about the lips; a constant low muttering delirium set in, and always about horses; subsultus, sinking in the bed, &c. In this state the patient lived till the thirty-fourth day. On examination all the lower part of the ileum intestine was found extensively ulcerated; and yet during the last nineteen days of the patient's life, the bowels had been so confined that aperient medicine had to be given.

The case presented three very distinct stages of fever. During the first eleven days, the symptoms were those of typhus; in the second stage, lasting four days, enteric fever was present, with the characteristic spots; while the third, lasting nineteen days, was marked by a copious rash, and all the ordinary symptoms of typhus.

Dr. Kennedy observed that he had published in the *Lancet* of 26th December, 1863, three cases of fever, in each of which the spots, supposed to distinguish enteric fever, were particularly well marked; and yet in none of these did any other sign of this form of fever show itself.—*March* 12, 1864.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.[a]

TWENTY-SIXTH ANNUAL SESSION, 1863-64.

Dr. Denham, President.

Dr. M'Clintock read a paper on *Missed Labour*, printed at page 51, Vol. XXXVII.—*9th January*, 1864.

Dr. Hardy *on Pregnancy with a Blighted Ovum.*

The regular and healthy progress of pregnancy is sometimes suddenly or slowly interrupted by the occurrence of various circumstances, which make such impressions upon the female system as result in the death of the ovum; accompanied at the time of their invasion, occasionally, by symptoms threatening miscarriage, such as hemorrhage, whilst at other times there are no such indications.

The histories of three cases, in which the ovum was blighted during pregnancy, I shall now detail to the society, which I hope may not be altogether devoid of interest.

In the first the ovum was blighted in the third month, and retained within the uterus until the end of the ninth; in the second it was blighted in the fourth; and in the third in the fifth month of pregnancy.

Death of the Ovum in the Third Month.—On the 23rd of December last I was requested by a medical gentleman to see a lady under his care, who had been suffering from uterine hemorrhage since the 22nd instant. I was informed that she had given birth to eleven children, and had three abortions. Being forty-four years of age, and not having had a menstrual period for six months, she considered she had passed the change of life, and this was principally what made her so anxious at the present loss of blood. She was a thin, pale, delicate-looking woman, and appeared considerably out of health.

On the day I saw her she had pains recurring at intervals. The uterus could be felt behind the pubes, and the os was dilated to the size of rather more than a shilling. I passed my finger into its cavity, and felt a substance of a fleshy consistence, which was evidently about to come away. I now learned that in July last she had considered herself three months pregnant. She had gone in a boat, at this time, with a party of pleasure, and had met with a great fright, which was followed by hemorrhage on the next day, and for a fortnight after. She felt perfectly certain that nothing but blood had been expelled.

[a] These Reports are supplied by Dr. Geo. H. Kidd, Secretary to the Society.

From this occurrence the uneasy sensations of pregnancy, which had troubled her, such as nausea, morning sickness, and waterbrash, ceased, and her breasts, which had been tolerably plump, entirely diminished in volume.

The uterine tumour she perceived to be as large as before, but in its locality there was a sensation of weight and soreness which quite incapacitated her from walking, until within the last three weeks.

In a few hours after my visit the ovum was expelled. It had the appearance of having been closely compressed; within its cavity there was a quantity of membrane, with a pulpy looking substance, which may have been the remains of the fetus partly absorbed.

The recovery of the patient was very slow; for a long time there was a good deal of uneasiness in the left uterine region, running down the thigh, and her general aspect was a look of extreme debility.

Death of the Ovum in the Fourth Month of Gestation.—I was summoned to a lady, five months advanced in her third pregnancy, on the morning of the 13th of last December, who had been affected with pains during the night. On my arrival she had just expelled an ovum, which had the appearance of having been some time dead. The membranes were yellowish and unbroken, and the fetus was about the age of four months.

I was informed that a month ago, on passing through her hall, she had fallen over some luggage. At the time she felt hurt, but afterwards thought no more of the circumstance, with the exception of some pain in the uterine region, where she had been struck in falling.

Her breasts, since the accident, had become quite flaccid; and she had not perceived any movements of the fetus. During her recovery the tenderness of the uterus, where it had been hurt by the fall, required the application of leeches and poultices, for its removal; and her breasts became so full and tense, that it was necessary to keep them covered with cere cloths until the distension had subsided.

Death of the Fetus in the Fifth Month of Pregnancy.—On the 3rd of October, 1862, a lady, in the fifth month of her second pregnancy, on rising off a music-stool, upon which she had been seated at a piano, owing to the pressure of a crinoline, knocked it down, and fell on the end of one of its legs, in a sitting posture, which hurt her so very much that her servant had to carry her up stairs. I was immediately sent for. I found her laid on her bed, pale, and nearly pulseless from very copious hemorrhage. The leg of the stool had lacerated the right labium, at its inner edge, very extensively. It required some time to restrain the bleeding and to restore the patient's strength.

From the date of the accident no fetal movements were felt, and stethoscopic examinations failed to hear the fetal heart pulsations. The uterine soufflet was always distinctly heard. The uterus corresponded in volume to the date of pregnancy; to the patient it felt like a dull weight. The

breasts diminished in size, and lost the characteristic appearances of pregnancy observable about the nipples at this period. This state continued for nearly three weeks. The volume of the uterus then felt of a globular form, becoming, occasionally, tolerably firm. The breasts enlarged, and the areola was distinctly turgescent and well marked. In two days after the fetus was expelled—being the twenty-first day from the accident. The head presented, and the child seemed some time dead.

Dr. Ingleby mentions a case very similar to the first I have detailed. The woman expressed her conviction of being three months pregnant. Soon afterwards she lost every symptom (amenorrhea excepted), but persisted in asserting its existence. At the ninth month she expelled a fetus of apparently three months. In her case there was no uterine hemorrhage that is mentioned.

When the ovum becomes blighted during pregnancy it may remain a considerable time in the uterus, giving rise to a train of anomalous symptoms which render patients doubtful as to their situation. Where they have suffered from hemorrhage, as in the present instance, those doubts are more likely to arise; but the distinct statement of the patient (Case I.), that nothing but blood was expelled, in a great measure assisted in the diagnosis.

It is a matter of very great importance to see the patient soon after the ovum is supposed to have died, as exemplified in the third case, that of lacerated wound of the labium. Should the fetus be retained the practitioner has opportunities of observing the fading away of the areola, the diminishing of the breasts, and of the various other symptoms which indicate the existence of a healthy pregnancy. It is well to be remembered, then, in cases of this description, where severe loss of blood was sustained, the state of the breasts is sometimes sensibly affected. An instance of this kind occurred in the practice of Dr. Ingleby. He was called, in consultation, to a lady within a short time of the full term of pregnancy, who had placenta presentation. Loss of blood had produced a marked impression upon the system, and her breasts, which had been previously much increased in volume, became soft and small. No movements having been felt, the child was supposed to have died; but this was not so, and, notwithstanding the interference necessary in the delivery of such cases, it was born alive, a fortnight from the first attack of flooding.

The late Dr. Montgomery mentions his having been called, on the 9th of August, 1840, to see a lady, who, in consequence of some domestic dissension, was just then separated from her husband, and was about sueing for a separate maintenance. Her being pregnant, as she positively professed to be, would very materially influence the result. She had felt fetal movements on the 7th of August, two days before his seeing her. His opinion was that she was pregnant; but he had doubts. Eight days

after he visited her, with another practitioner. The evidences of pregnancy were then less distinct: the abdomen was smaller; the mammæ more flaccid, and the areola less. The medical man in consultation with him considered her not pregnant.

On the 15th September the evidences of pregnancy were still less; and on the 6th of October they were completely gone. On the 17th a dead fetus was expelled.

In my third case the breasts diminished and the areola faded from the time of the accident, so that the mammary symptoms were quite removed until within about two days of expulsion of the fetus. Had any one seen them, on one of those days, who had not observed the change in them I have mentioned, I think he should have felt considerable doubts as to the condition of the ovum. I do not remember, on any previous occasion, having remarked so rapid and striking an alteration produced, or one so likely to embarrass.

This excitement of the breasts was of very short duration, and subsided without giving the patient any inconvenience, contrary to Case II., in which cere cloths were necessary.

In the case of an ovum becoming blighted, and producing symptoms such as were present in the first case detailed, a patient at her time of life has good reason for feeling anxious, when it is remembered how frequently diseases of the uterus, of malignant character, are then becoming developed, and are sometimes far advanced in their progress when hemorrhage, for the first time, has directed attention to them. To illustrate this remark I need only mention the very insidious advances sometimes observed in carcinoma.

But even where there is no complication or disease, the presence of a blighted ovum within the uterine cavity for so long a period as six months is very liable to give rise to a train of unpleasant symptoms—cessation of menstruation; irregular and unhealthy discharges; weight and uneasiness in the pelvic cavity; inability to walk, and the risk of again becoming pregnant, with the remains of a dead ovum in the uterus, of which condition a case is recorded by the late Dr. Montgomery.—*13th February*, 1864.

Dr. Hardy *on the Use of Cere Cloths in the Treatment of Painful Affections of the Female Breast.*

I mentioned having applied cere cloths to relieve the distension of the breasts in one of the cases detailed in the foregoing paper. Made as they usually are, of bees-wax and sweet oil, spread on linen, they are ill suited for the purpose for which they are intended; they become very brittle and crumble about the patient's bed. In preparing them if a little glycerine and resin plaster be added to those ingredients, and spread on

thin leather instead of linen, they can be removed and reapplied without any inconvenience.[a]

By spreading over the surface occasionally, glycerine diluted with tepid water, this application may be made to answer the purpose of a perpetual vapour bath on the breast, and thus prevent the often injurious handling and rubbing which it is frequently subjected to from the nurse, or save the risk of cold in fomenting.

By saying so I do not mean to detract from the very beneficial effects of both hand rubbing and fomentations, when properly applied. It is their injudicious and wrong application which I condemn.

Cere cloths are very soothing in painful affections of the breasts of very young infants. I lately applied them in a case of inflammation and enlargement, where the child was about a fortnight old; both breasts were greatly distended. The nurse, when showing them to me, said she had not squeezed them until she first let me see them. No other means were resorted to, and in a few days the disease was quite removed.

In painful and irritable conditions of the breast, either on the setting in of lactation or owing to disease, the addition of extract of belladonna is sometimes most beneficial. It may be spread over cere cloths made as I have described, and in this way be rendered more efficacious; as then it will be prevented from drying on the surface. In two cases particularly I found the application of belladonna most valuable.

In March, 1852, I attended a lady in her first confinement; she was remarkably healthy, and had a good labour. Her breasts were well formed, the nipples very good, and without any trace of tenderness on the surface; but so exceedingly sensitive that the least touch, in endeavouring to nurse her infant, was intolerable.

The extract of belladonna was smeared over her breasts outside the margin of the areola; one application left on for a short time (less than an hour) removed every trace of irritation. She has given birth to several children since then, and never again had any such annoyance.

I took the precaution, which is very necessary in all such cases, of strictly removing the infant from its mother until after the belladonna had been some time very carefully washed off the breasts.

The second case was a lady who was confined of her first child in November, 1856. During her labour, until she inhaled chloroform, she was most violent. She then became so quiet, her husband, wondering at

[a] *Cere cloth.*

℞.—Cer. Flav.
Ol. oliv. a, a, ʒii.
Glycerine ʒii.
Emp. Resin, ʒss.—M.
Ft. Emplast.

Cere cloth with Belladona:
To the above add Emp. Belladon. ʒss.
Cere cloth with Opium:
To the cere cloth plaster add Emp. opii. ʒss.

the sudden return of peace, looked into the room. On perceiving him she said :—"Dick, I don't mind it now, I have the chloroform."

On her breasts becoming distended at the commencing of lactation she suffered such intense pain that I was sent for during the night. She was in a great state of excitement.

In addition to internal remedies I smeared her breasts freely with extract of belladonna.

Next morning I found her perfectly tranquil. She said (pointing to her breasts):—"Here they are, like two stones; but not in the least painful;" nor were they from that time. This lady and her sister had the same peculiarities; their breasts became intensely hard, but without any secretion of milk whatever. Neither of them could nurse.

The greatest degree of this peculiar sensitiveness I ever met with was in vaccinating two gentlemen, brothers, both strong healthy men. Neither of them could bear the least touch of the lancet without exclaiming. One of them in particular became deathly pale; the perspiration poured down his face, and he had to be laid fainting on a sofa. Wine and smelling salts were freely used before I could complete the operation, which occupied not less than twenty minutes.

It was only necessary to scratch the skin lightly with the nail to cause the same effect as that excited by the lancet.—13*th February*, 1864.

Mr. M. H. Collis detailed the particulars of a fatal case of ovariotomy.—12*th March*, 1864.

Dr. Hardy exhibited a fatty tumour, and read the following account of it:—

I removed, this morning, a fatty tumour from the right labium of a patient under my care in Steevens' Hospital. The woman has had no children, though an extensive laceration of the perineum would lead one to suppose she had. She cannot account for the laceration. She is about thirty-six years of age. The tumour was growing twelve months. It caused no painful sensations, excepting by the dragging of its weight. In the longitudinal circumference, from above downwards, it measured 9 inches; its smallest circumference is 7 inches; and its weight $4\frac{3}{4}$ ounces. I removed it by the écraseur, the patient being under the influence of chloroform. There was not a drop of blood lost. On examining the tumour, it will be seen to contain no blood-vessels, except in the integumentary covering. In applying the écraseur I drew the tumour firmly through the chain of the instrument, so as to allow sufficient covering of integument to the labium, and to avoid including the mucous membrane. In consequence of this precaution I expect the wounded part will heal with less uneasiness to the patient.

Fatty tumours of the labium are not so frequent as those of a fibrous character. As mentioned in the history of this case, they give rise to few symptoms, and seem only by their weight and size to give annoyance. Gradually they become pediculated when very large; but this state had not been arrived at in the present instance. The tumour here was, as the excised part shows, of considerable breadth, and closely attached to the integument of the labium. These tumours grow more frequently from the right than the left labium. Sometimes they become excoriated and ulcerated on their inferior surface, owing to friction, it may be, or in consequence of acrid vaginal discharges passing over them. They do not seem peculiar to any particular age. They sometimes very much resemble encysted tumours; where any doubt exists as to their character, the exploring needle will be of valuable assistance.

Removal by the écraseur has decidedly very great advantages in those cases over every other form of operation. It is so very soon over, the risk of hemorrhage (in this locality so often very free) is greatly diminished, and the size of the wounded integument is so much contracted.—16*th April*, 1864.

Operations for Ruptured Perineum.—Dr. Denham read the following paper, and exhibited a doubly-curved needle that he had made for inserting the deep sutures in the operation:—

The subject of prolapsus uteri has, at all times, been one of interest and perplexity to the practitioner, too often entailing an amount of suffering and distress upon the patient that only terminates with death. It is not my intention to enter into the history, the different causes assigned, or the different modes of treatment recommended for the disease, but simply to detail the result of some cases treated in the manner recommended by Mr. Giddings, Mr. Baker-Brown, and Mr. Savage.

We were induced to try the operation by meeting a patient sent up from the country, who could not retain a pessary of any size or form that we could think of; and again, by meeting with a second at the same time, who could not bear the pain and irritation of any instrument sufficiently large to afford her relief.

We have learned, however, that the operation, although safe and easily performed, is not suitable to many forms of prolapse; and further, that it will sometimes fail; and, as my friend Dr. Churchill quaintly remarks, the uterus, during its active life, will gradually bore its way through, and again protrude.

E. S., a stout active woman, aged thirty-eight, sent up to hospital from county Galway, by her master, to whom she had been a faithful servant for many years. States that she is the mother of three children; was thirty hours in labour of her first child; had a slow recovery, and was long delicate; had a feeling as if something was about to come out

through the passage, but the uterus did not protrude until after the birth of her second child. Her third and last child is now three years old, and she has suffered from prolapse ever since its birth. She declared she was often obliged to go on her knees, and then lie down on the stairs while at her work as a servant. She suffered often and much from retention of urine; and so low did the womb come down, that when attempting to pass water, it would touch the bottom of the chamber pot. On examination, we found the walls of the vagina very much relaxed, and the perineum badly ruptured. After keeping her in bed for some days, and using astringent lotions, we tried several varieties of pessary—the large globular, the flat, the ring, and Hodge's, but all to no purpose; she could not retain them; either while walking across the floor, or on going to stool, they were sure to escape; and with the pessary down came the uterus.

The dread of an operation—the fear of never coming *to*, as she expressed it, if put under the influence of chloroform—rendered a large amount of persuasion necessary to induce her to comply with our wishes. However, we operated, on the 29th October, and she left the hospital perfectly well on the 6th of December last—having, for many days previous to leaving, gone, not only up and down the hospital stairs, but out into the city for hours, without any inconvenience.

We have heard from her since her return to the country, and she continues, so far, comfortable and well.

Rose M'E., the wife of a small farmer, came from Kells, for medical advice, in October last. She appeared pale and delicate; aged about forty. She stated that she had married late in life, that she was the mother of two children, and that her health had been very bad indeed since the birth of her first child. Her labour then was long and severe, and the delivery was completed by instruments. From the time of her first confinement she suffered from a sense of bearing down, and experienced pain and difficulty in walking; but the womb never came down entirely until after the birth of her second child, two years ago. Since then the womb has been very much down. She has been unable to walk much; the bowels have been either too much relaxed or obstinately constipated; the appetite had grown weak and debilitated.

On examination, it was found that the whole perineum was destroyed, and even the lower end of the bowel, throwing the two passages, for a short distance, into one. On dissecting off the integument, the fibres of the deep sphincter ani were brought plainly into view.

She was operated on on the 13th November last. The extent of skin and mucous membrane removed was necessarily greater than in the former case, and consequently there was greater loss of blood, but not to the extent of creating any alarm. The deep sutures were removed in sixty hours, and the superficial in ten days.

She left the hospital, well, on December 5th. I have seen her since, much improved in health, the wound perfectly whole, and the uterus in its place.

Miss R., aged 45, came into hospital in December last, labouring under prolapse of the uterus. She stated that three and a-half years ago, while lifting a feather bed, she felt as if something gave way; and, on examination, she found something protruding, which proved to be the uterus. A pessary was introduced soon afterwards, and allowed to remain for two and a-half years. At the end of that time it produced such an amount of irritation and inflammation, that she was obliged to have it removed; but the instrument was so embedded in the soft parts that very great difficulty was experienced in its removal; and so tender did the parts remain, that she never could bear an instrument again; and at the time of admission the uterus had been down continuously for months.

The operation was easily and safely performed on the 18th December; but vomiting set in twelve hours after, and continued incessantly for twenty-four hours, much to the distress and discomfort of the patient, and the alarm of the medical attendants, as it was feared the sutures would give way. They did not, however, and the wound healed entirely and rapidly, the deep sutures having been removed in fifty-seven hours, and the superficial in ten days. She remained in hospital for some weeks, apparently quite well, and the wound perfectly healed; but unfortunately, on the very day she returned to her room, being of an excitable fussy disposition, she over-exerted herself, and brought on a violent fit of retching, during which a portion of the wound gave way, and the uterus again made its appearance. She did not return to hospital, or tell us anything of what had happened, but went out continually about some pressing business of a pecuniary nature for ten days, at the end of which time she was nearly as bad as before the operation. She is now in hospital again, and we are greatly at a loss to determine what to do for her, or how to relieve her. Any hint on the subject, from the members of the society will be most thankfully received.—16*th April*, 1864.

Dr. M'Clintock read a paper, entitled, *Further Observations on Missed Labour*, printed at page 307, Vol. XXXVII.—16*th April*, 1864.

Memoir on the Pathogeny of Strabismus. By PROFESSOR F. C. DONDERS, of Utrecht. Translated by E. PERCEVAL WRIGHT, M.D., F.R.C.S.I., Surgeon in charge of the Ophthalmic Dispensary of Steevens' Hospital.[a]

(*Continued from* Vol. xxxvii., No. 74, page 490.)

IN the majority of instances strabismus ends by becoming habitual. The rule is that one and the same eye always deviates (strabismus simplex). This was already the case when the strabismus was still periodic. If the strabismus alternates in a hypermetropic patient, other causes are frequently at play. The strabismus is generally concomitant; the movements are free; the area of movement is normal save that there is an excessive mobility inwards, and a more limited one outwards in both eyes. The one and the same eye alone deviates; the other is invariably well directed. Notwithstanding the internal recti muscles must be considered as shortened. This shortening, at first dynamic, in the case of constant strabismus becomes organic. It is the consequence of excessive use combined with feebleness of the antagonistic muscles; there is no pathological change present. That both the internal recti muscles are shortened results from the habit of keeping the object intently on the side of the distorted eye, so that even in the normal eye the internal rectus is brought into relatively strong contractions. In this position the hypermetropia of the non-deviating eye is best overcome. Even when squint has become habitual, there is in connexion with fixation a relatively stronger contraction of the internal recti muscles whereby the angle of squint increases; in the case where there is already a large angle of deviation, this will be only in a slight degree, because their increased straining occasions less motion. After tenotomy the increase of convergence in these cases, while regarding some object intently, appears again very strongly. This increasing of convergence in the act of fixation, after a correct position has been obtained by tenotomy, is important from a two-fold point of view. First, because we have adults who declare that they distinctly feel the convergence, and perceive, at the same time, that they induce it, as it were, voluntarily in order to see more distinctly. A more direct proof that hypermetropia may determine strabismus could, I think, not be given, for here we have, in a manner, a return to the first period of strabismus, with this difference—that the deviation can now be appreciated by the person himself; and, just as in the original stage, the first appearance—so here the relapse must be guarded against by the use of convex

[a] This Translation is made from the original Memoir in the *Archiv für Ophthalmologie* Herausgegeben von Arlt, Donders, and A. von Graefe. Band. 9, Abth. 1. Berlin, 1863. With a few additions from the French Translation by Dr. van Biervliet, in the *Annales d'Oculistique*. Tome L, 3e and 4e Livraisons. Octobre, 1863.

glasses sufficient to centralize the hypermetropia. The practical direction to use proper convex glasses while working, after tenotomy, is the second reason why we considered the symptom in question important. In simple strabismus the sharpness of sight becomes, in the affected nerve, more and more enfeebled. At first if the hand is held before the normal eye, the squinting eye directs itself properly towards an object, and may even remain thus fixed when the hand is removed; but it soon, however, gives place to the other, usually when some new motion is attempted, often even at the first winking of the eyelids. Sharpness of sight will now have already diminished in the deviating eye; but it is still, for a considerable length of time, nearly sufficient, and it may, by exercise, be restored; it improves, too, most generally, immediately after tenotomy. In the course of time, however, the deviating eye no longer directs its line of vision on the object when the normal eye is excluded; but this line passes along on the inner side, so that the image of the object falls on the inner side of the retina. When such is the case we may conclude, that the sharpness of sight of the deviating eye in the line of vision, as well as in the common visual field of both eyes is diminished; whilst, on the other hand, that of indirect vision on the innermost portion of the retina, so far as this portion has its own field of vision and sees objects not represented in the retina of the other eye, has remained undiminished.

Von Graefe was the first to investigate this loss of physiological sensibility by psychical abstraction. Truly a remarkable phenomenon! It is a well known fact that we can, by attention, increase the acuteness of our organs of sense. That which precedes proves, on the other hand, that a nerve may lose the power of receiving those impressions from which we desire psychically to abstract ourselves, and presents us, therefore, with a very important example of this fact. In such cases, although no organic changes are to be noticed in the retina, yet, neither by constant practice nor by tenotomy, is there any considerable improvement to be obtained.

Has the origin of strabismus been accounted for on any former occasion by the presence of hypermetropia? This question may, I think, be answered in the negative; indeed it is almost natural that it should be so, because it is only for the last few years that hypermetropia has been understood, and total hypermetropia, and, to a considerable extent, latent hypermetropia had been overlooked, until that, having convinced myself of their existence, I commenced to perceive their connexion with strabismus. My perceptions have been, without any doubt, assisted by what has been discovered and recorded by my predecessors. Not to speak of some few isolated observations[19] which, since the discovery of hypermetropia, proved it to exist along with strabismus, I would, in the first place, refer to Böhm's[20] investigations

respecting squinting, where it is plainly stated that squinters can distinguish a certain type, with the assistance of *convex glasses*, at a greater distance than they would with unassisted vision. This observation was of great importance, and it might have led to the discovery of hypermetropia, and, in particular, of the connexion of hypermetropia with strabismus, had Böhm, with a thorough knowledge of dioptrics, understood and rightly explained the facts which he had observed. Instead, however, of thinking of a condition in which the focal point of the dioptric system should lie behind the retina, Böhm had recourse to a mysterious connexion of "*physical presbyopia*," with "*vital myopia*;" and he was very far, at all events, from seeking in this for the cause of squint. In investigating the origin and cause of strabismus he falls into the same error as all his predecessors—investigating them, not for a distinct form of squint—for a real form of disease—but for a symptom—squinting in general. The causes, resulting from totally different conditions are then sought for and investigated indiscriminately. It might have been forseen that the pathogeny of squint could not in this way be revealed. And further, when Böhm treats of the "*origin of squinting having its source in the eye itself*;" and also, of the "*ætiology of squinting as originating in the eye*," the condition of the deviating eye is, and remains with him, the important matter; he speaks of the "short-sightedness of one eye, while the other preserves its normal condition," of "asthenopia of one eye," of "feebleness of vision of one eye;" and, according to him, it is this eye always that deviates. In this deviation he sees an effort to exclude this eye from vision, without recognising in it an effort to try to improve the distinctness of the images of the undistorted eye. That which we have admitted farther back as one of the circumstances under which the eye more easily allows itself to be withdrawn from binocular vision, is, for him, the all important cause of squint. So Böhm was as far from understanding the reason of converging strabismus as any one else.

In later years von Graefe[21] was certainly very nearly discovering the cause of strabismus to be hypermetropia. He did not, however, exactly treat of the pathogeny of squint; and he even apologises when, in passing, a few remarks respecting it escape from his pen. These remarks, however, we regard as well worthy of attention; but, as it did not occur to him to introduce hypermetropia as an element into the subject, they remained, for the most part, fruitless. Von Graefe mentions, as a well-known fact, that an "intercurrent or periodic squint" frequently, if not generally, precedes a permanent one. Further on, he remarks that all cases of intercurrent squint do not pass into permanent squint. So long as this has not taken place, these cases would require some special remarks, from the point of view of operative assistance. For this reason von Graefe mentions them. But he has obviously also felt that the symptoms incident to these cases had a peculiar significance as

regards the investigation of their pathogeny; at least, he considers, from a pathogenetic point of view, the three following categories distinguished by himself:—

First Category comprehends those patients whose eyes, while in a state of repose, show no appreciable deviation; but do so the moment they fix them steadily on a certain object, whether it be near or remote. Von Graefe thinks there is a possibility that a cause of disturbance might result to binocular vision from the deviating eye; and that, therefore, its image might be voluntarily deviated. That, in this way, a permanent squint might possible arise is plain—Von Graefe does not, however, consider everything as hereby explained. "If the deviation manifests itself only under certain conditions, for instance, in the case of a vivid perception of the images on the retina, and not where this fails, one must suppose the existence of some link between the act of vision and the muscular movements" (von Graefe, l. c., p. 281); and again—"if this latter is not the disturbance of the images on the retina of either side, then, in the next place, the circumstances of the accommodation present themselves for investigation."

With respect, however, to the first mentioned supposition, von Graefe remarks that the one eye deviates while intently regarding an object at any distance, and even when the hand is placed before it; and whilst it did not occur to him, with respect to the last mentioned case, that hypermetropia might account for it, so he knows no other explanation to give save the following somewhat obscure one:—"Every act which has for its aim the exact perception of objects throws on the altered muscle the incentive to abnormal contraction.

Second Category comprehends those cases in which, up to a certain point (eight inches, a foot, four feet), the axes of vision have a normal direction, whilst beyond that distance a striking deviation is manifest. Such cases are generally connected with myopia.

Third Category comprehends cases of which he says "that the pathological convergence appears only in the case of accommodation for near objects. The symptom appears just as well marked when the squinting eye is covered, and must, consequently," he says, "have its origin in the condition of the accommodation, probably in an increase of the resistance of the muscles, accompanying an increased state of the refraction of the eyes. The increase of the tension of the muscles," he continues, "arouses the slumbering impulse to abnormal contraction in the affected muscle."—He further refers to the peculiar cases in which, while looking at near or distant objects, converging strabismus arises; but in which, when looking at objects at a moderate distance binocular vision is preserved. He explains this as partly due to myopia; but frequently both presbyopic and hyperpresbyopic patients belong to this category; myopia in distans sometimes too appeared to exist. He sums up as follows:—"A feeble degree of

pathological convergence of the lines of vision, corresponding to the natural tendencies to straining of the muscles, exists, for all and every distance of the object. If the refraction of the eye be augmented, either by bringing the object nearer or holding up a concave glass, then the morbidly increased contraction commences; for moderate or short distances, and relatively large retinal images, the prevailing tendency of the muscles is, in order to preserve single vision, resisted; for a greater distance, the retinal images decreasing in size, this no longer happens, and double images arise which, owing to an abnormal contraction of the muscles, appear at greater distances from each other."

Finally, Alfred Graefe,[22] in a case of intermittent squint—called also, but less correctly, "spastic squint"—asks himself the question whether "It is the circumstances of accommodation which occasion the deviation of the right eye?" and replying to it as follows:—"Certainly not; for it has been expressly pointed out, at the commencement of this chapter, that the deviation always commences as soon as an object has been steadily looked at, and consequently it is totally independent of the state of accommodation prevailing at the time"—convincingly proves that he did not think of hypermetropia, which itself requires a straining of accommodation while looking at distant objects

From all this, it is evident that hints were not completely wanting in literature, tending, after the slighter degrees of hypermetropia were detected, to connect it with strabismus.

III. DIVERGING STRABISMUS.

Diverging strabismus depends, as a general rule, on myopia. At the commencement of my statistical researches I was already aware of the relations which existed between hypermetropia and converging strabismus; but I was very far from supposing that there was at all the same intimate connexion between diverging strabismus and myopia. It was systematic investigations alone that brought this fact first to light.

The nature of the relation is not altogether the same in both cases. If hypermetropia determines the converging squint, it is because the anomaly of refraction requires great efforts of accommodation. When diverging strabismus arises in connexion with myopia, then the anomaly of refraction as such, is not altogether without direct influence, but the chief cause of the deviation depends on the distension and change of form of the globe of the eye. When myopia exceptionally occurs, owing to a flattening of the cornea, strabismus is not to be looked for. Since Arlt[23] demonstrated, by anatomical researches, that ordinary myopia depends on the extension of the membranes of the posterior portion of the globe, and the lengthening thereby of the optic axis, this important fact has been generally admitted as correct. All the diameters of the globe are increased in myopia, but especially that which corresponds to the optic

axis; it follows that the globe has a tendency to take the form of an ellipsoid, of which the major axis is the optic axis. These large diameters may diminish the mobility of the eye in general; this is especially caused by the ellipsoidal form, which opposes a much greater resistance to its rotation on its minor axes in the interior of a cavity of the same shape, because this demands a change of form. The centre of motion is not only at a distance from the anterior, but also from the posterior pole of the eye; its position is not very unfavourable.

The researches which I made conjointly with Dr. Doyer, establish that the centre of motion is situated behind the centre of the optic axis, so that the portion of this axis situated in front of the centre of rotation is to that behind, as 15 : 11. (*Vide* the annexed table given from the French.)

TABLE of the mean obtained by the examination of 19 emmetropic eyes.

19 myopic eyes, M = 1 : 16 up to M = 1 : 2·25

12 emmetropic eyes, H = 1 : 16 „ H = 1 : 3·75

	Mean length of the visual axis	Situation of centre of motion			Angle *a* between the axis of cornea and line of vision
		Behind the cornea	In front on the posterior of the sclerotic	Proportion per cent.	Behind the middle of the visual axis	
	Millimètres	Millimètres			Millimètres	
E	23·53	13·54	9·99	= 57·54 : 42·46	1·77	5°·082
M	25·55	14·52	11·03	= 56·83 : 43·17	1·75	2°
H	22·10	13·22	8·88	= 59·8 : 40·2	2·17	7°·55

About the same proportions, however, were also found in the case of the longer axes of myopic people; here, consequently, the centre of motion lies further away from the posterior surface of the sclerotic. The excursions are therefore, in these cases, for equal degrees of rotation larger, and the movements necessarily limited; this limitation would be still greater were not the entrance of the optic nerve, on account of the disproportionate extension of the external posterior portion of the segment, moved further inwards, and thus removed considerably less from the centre of motion. To this limitation, moreover, the greater distance between the centre of motion and the insertion of the muscles may help to contribute, to which distance, the arc of rotation, obtained with a given shortening of the muscles, stands in inverse proportion.

Apart from the consideration of all this, the elongation of the axis of the eye gives of itself a sufficient explanation of the diminution of the movements of the globe; this latter affects the movements inwards as well as outwards. With myopic people it is so general that out of seventeen eyes examined, nine could not turn themselves sufficiently far so as

to bring, without modification, our method for ascertaining the centre of motion, into use—a method which requires an excursion of not more than 28° inwards and outwards. Diminished outward motion of the eye is primarily of no further consequence than that the lateral excursions of the eye, for binocular vision at a distance, are smaller, and that to obtain it, assistance, by turning the head, must be sought, a thing which is, however, always necessary when spectacles are worn; but insufficiency of motion inwards has other and more important consequences which we must consider consecutively, so as finally to see absolute diverging strabismus appearing as its ultimate result.

We take it for granted that there is an insufficiency of motion inwards, when the lines of vision cannot be brought to intersect at a distance of 2″·5, at which they cut one another at an angle of about 51°. In high degrees of myopia this insufficiency pretty nearly always exists, and to account for this a two-fold reason may be given. In the first place, the mobility is in reality impaired in consequence of the extension and altered form of the globe, and consequently the insufficiency may be considered absolute; but, in the second place, in consequence of the smallness of the angle α the axes of the cornea must, in order to make the lines of vision intersect, in the distance of 2″·5, be made to converge still stronger than in the case of emmetropic eyes; hence we see that motion inwards must be at least relatively, if not absolutely, confined.

The insufficiency of which we are here speaking leads, in some cases, to wearisomeness of vision, when the nature of the work requires for a lengthened period a certain amount of convergence (asthenopia muscularis).

I have met with some cases in which, in the first instance, vision was binocular; but when the one eye became fatigued it deviated, and then the vision could be continued with facility; others in which this deviating was felt as an annoyance, and gave rise to complaints. I found this latter occur when the degree of myopia was comparatively small, and where, in addition to the muscular resistance of the eye, a certain weakness of the muscles (not merely insufficiency of motion, but actual insufficiency of the internal recti), had to be assumed, a condition which I have observed to be hereditary in cases of moderate degrees of myopia with the before-mentioned symptoms. This deviation, produced by the continuous effort to enjoy binocular vision, is the first degree of relative diverging strabismus. The lines of vision intersect each other normally for great distances, but for objects close at hand one eye only is used.

Relative diverging strabismus is here considered as a consequence, and, in some measure, as a further development of the insufficiency of motion inwards; this is true up to a certain point. If we try, however, to define this relative squinting, it then appears that it is inseparable from high degrees of myopia; and that even if, in such cases, motion was not limited,

the squint would nevertheless be present. Relative diverging strabismus takes place, in fact, when the close proximity of an object which one wishes to see distinctly, excludes binocular vision; it consequently makes its appearance, even in cases of unlimited convergence, the moment that the farthest point of distant vision lies nearer the eye than the point which can be reached by the strongest convergence. In this sense, when the myopia for example is greater than $\frac{1}{2{\cdot}5}$ relative diverging strabismus is necessarily present, one eye is certainly (cases of converging strabismus excepted), if the person at the time sees distinctly without glasses, always directed outwards.

From what has been stated it will be seen that relative diverging strabismus may arise, on the one hand, in cases of considerable insufficiency of the internal recti, myopia being completely absent; and, on the other hand, in cases of high degrees of myopia, without any insufficiency of the muscles. The fact is, it occurs in its most important forms, when myopia and insufficiency of the muscles are both present in a moderate degree. Myopia may be considered as the starting point. If it be wanting the muscular insufficiency will only lead to muscular asthenopia, and will seldom develop itself into diverging strabismus. If myopia be present, then a number of circumstances conjoin to bring on relative diverging strabismus, and by this very means prevent muscular asthenopia.[24] The explanation is simple, and has already been given when treating of muscular insufficiency; myopia requires a greater convergence of the lines of vision, because the object of sight lies nearer the eye, and it is exactly in myopia that convergence is, for two reasons, more difficult: first, because of impeded motion, and secondly, because of the altered direction of the line of vision (the smaller angle α). That relative diverging strabismus chiefly occurs in cases of myopia is herewith explained; to this must be added, that the want of binocular vision and the aversion to double images furnish, in this instance, no considerable counterpoise. It is generally some small object that a myopic person wishes to see distinctly; this object he brings close to one eye, while the other eye is directed towards some remote object which, in consequence of the myopia, present very diffuse, and hence, only slightly disturbing images. If the act of vision occurs with one eye turned aside, then so much the less will there be a strong tendency to convergence, because then the distance of R (the far point of distinct vision) at once becomes smaller, and the object must consequently be held still closer to the eye. It is precisely in cases of difficult convergence that the combined efforts of accommodation become particularly great.

In progressive myopia we often see how that binocular vision tries to maintain itself in opposition to relative diverging strabismus; but the fatigue which is always increased by the exertion, causes it for the most part constantly to succumb. Reading, for example, commences

binocularly; but after a little, one eye turns away, involuntarily and unconsciously, so that one is told that one page shifts itself over on the other; numerous transitions may be proved. If an object be brought closer and closer to the eyes, then the convergence increases almost to its maximum; and if the object remain so, then the one eye turns off the more quickly in proportion as the object was brought nearer to the maximum of convergence. This will equally occur in cases of strong convergence, when the one eye is covered with the hand; and, if the covering hand be removed, the convergence nevertheless continues. Even when the object is brought close to the one eye, whilst the other, still kept open, is covered with the hand, a sufficient convergence is but rarely effected; the effort to maintain binocular vision, begun at greater distances, was the condition on which the convergence was effected. It is not to be found even in the case of a relative diverging strabismus which has become permanent. As an instance of a transition to this latter, it is observed that when the eye is fatigued, convergence is absent, but shows itself whenever it is restored by repose.

Between periodic and permanent diverging strabismus there exists still another condition of some practical importance to which I have already called attention.[25] In it there still exists a tendency to convergence, this is seen by approaching an object near the eye; but before the farthest point of distinct vision is as yet reached, or at the least very soon after, the one eye turns away. In such a case, by the assistance of a concave glass, the object is once more seen with both eyes; frequently, however, complaints of fatigue coming on will now be heard; and experience teaches to look for its cause, not in the effort of accommodation, but in the requisite convergence, even if this be comparatively slight; consequently muscular asthenopia is in existence; and here, in order to make binocular vision possible, the combination of prismatic with the concave glasses becomes necessary. In such cases it is particularly obvious that we must seek for the cause of the relative diverging strabismus in the impeded motion inwards, whilst the tendency to use both retinæ for the sake of binocular vision may continue undisturbed. It is only in the case of an absolute diverging strabismus that this tendency, as will appear, is not unfrequently lost.

We have seen above, that in progressive myopia binocular vision of near objects, can only with great difficulty be retained. There are, however, exceptions. According to von Graefe—"an energetic co-operation of the internal recti is consequent on relative myopia;" he even goes so far as to assert, that it is to be regarded as a *pathological condition* "when the contractibility of the internal recti does not progress harmoniously with the increase of the refraction (myopia)."[26]

Indeed, even in cases of high degrees of myopia, whether in consequence of a favourable size of the globe of the eye, or in consequence of an

original or acquired preponderance of the internal recti muscles; the lines of vision may sometimes, while looking at near objects, be correctly placed, and may, without much effort, be kept in that direction. This mostly happens at the expense of mobility outwards. A limitation of mobility is, in such a case, never wanting, and it may reach such a degree, that in looking at distant objects the lines of vision cannot be brought into a parallel direction; relative converging strabismus will now present itself. The cases mentioned at page 9 have had their origin alluded to; even in these cases convergence is not sufficient, and then the strange combination arises of relative diverging strabismus while looking at near objects, and of relative converging while looking at those which are distant; whilst looking at moderately distant objects binocular vision is retained. This reminds one of the combination of myopia with presbyopia; all such cases, however, are exceptional. The rule is, that facility of convergence does not keep even pace with the development of the myopia, and that, in a very short time, the tendency to relative diverging strabismus becomes perceptible. I convinced myself, by investigations, that in myopia, if it commences with parallelism of the lines of vision; mobility inwards is generally, in the majority of cases, diminished,[27] whilst the mobility outward suffers no diminution whatever; nay more, the lines of vision, under the influence of a prism, can usually be brought to a greater divergence than in the case of non-myopic people. It would appear as if convergence was easier on the part of myopic people, since with them, according to my determination of the relative extent of accommodation, certain degrees of convergence are possible, without a proportionate effort of accommodation; herein, however, there is no absolute proof. We should simply learn from this, that by practice, the movements of accommodation can isolate themselves to a certain extent from the efforts of convergence, and make themselves apparent only in cases of high degrees of myopia.

Absolute diverging strabismus is characterized by divergence of the lines of vision when viewing distant objects. While looking at objects close at hand, the divergence sometimes remains unchanged; sometimes it diminishes, or even gives place to a convergence of the eyes, this not being sufficient however; binocular vision is, in such cases, destroyed. I have observed in a few instances, that while looking at very distant objects, divergence existed, but that while regarding objects only a few feet or inches distant, this gave way to a sufficient convergence, which, however, could not be long retained. The fact is one worthy of notice; it may be explained by the circumstance that binocular vision is of much greater importance in judging of near than of distant objects. At first diverging strabismus is generally but little marked, and it increases but slowly; sometimes it will continue but slightly marked throughout one's whole life. It has appeared to me as if there was another origin, in addition to myopia, for the higher degrees of strabismus.

It is only the more absolute forms of diverging strabismus which are designated by the name squint; in this sense it is much less oftener met with than converging strabismus. Now, although here, as in converging strabismus, a certain number of cases must be referred to primary disturbance of the muscles (paralysis, inflammation, contraction, complicated congenital anomalies, &c.; a blind eye frequently also deviates outwards), yet myopia does not occupy the same prominent position as a cause of diverging strabismus, as hypermetropia does in relation to converging strabismus; at the same time, however, myopia is found in about two-thirds of the cases of absolute diverging strabismus. If we add to these cases those of relative diverging strabismus, then the diverging form of squint is quite as frequently met with as the converging, if not even more so, and the extraordinary causes, proceeding originally from affections of the muscles, or from blindness of one eye, retire completely into the back ground, for in at least ninety per cent. of the cases of relative diverging strabismus, myopia is found. It has often been remarked, that while converging strabismus is chiefly to be met with in children, diverging strabismus but rarely occurs until much later in life. The observation is correct; the fact is closely connected with the cause of its existence—*progressive myopia.*

Now, although absolute diverging strabismus is, as a rule, chiefly developed out of relative, yet in by no means every instance does the absolute follow the relative; the reason of this lies in the circumstances we have mentioned; and it seems indeed to be rather the exception. We find here, as in converging strabismus, a similar relation to the cause. Thus as the majority of hypermetropic people remain free from it, so it is also certain that many myopic people will be found with relative diverging strabismus, and yet the absolute form never developed therefrom. Here, then, the question likewise arises—what collateral circumstances operate to produce the true absolutely diverging strabismus?

Perhaps we shall be able to invert the question, if we, in the first place, reflect—why the relative distortion, in general, disposes to the absolute. The result of this examination may be thus formularised.

Relative diverging strabismus determines the formation of different images on the two yellow spots of the retinæ, at least in the case of close vision. The great desire that these impressions should correspond—the struggle for simple binocular vision, must, in general, be thereby very much weakened. A deviation commencing at the first attempt at convergence, at once becomes considerable, whilst the individual simply yields to the muscular impulse; perhaps this occurs, too, in order to remove (however unconsciously), the double images further from each other, or even to exclude the effort of accommodation, which is associated with troublesome convergence, and thereby remove the limit of distinct vision to a greater

distance from the eye. If, for example, in the case of blindness of one eye, the internal recti are no longer strained for the purpose of seeing near objects binocularly, then these muscles, on account of their diminished energy, lose the power of moving sufficiently, and the usual consequence is a diverging strabismus. Now, relative diverging strabismus induces a similar inactivity, the result likewise of impaired energy; these two important factors meet together, viz., slight resistance to double images, and diminished vigour of the internal recti.

It cannot, therefore, appear strange that the movements of these muscles, even in the case of distant vision, soon cease to be efficient. This must still more be the case in myopic people, in whom the angle α is particularly small, and where, consequently, distant vision requires a smaller divergence of the axes of the cornea than in emmetropic eyes. When once the action of the internal recti is weakened, the effort, which must be made, to overcome the tendency to divergence will easily bring the point of farthest distance nearer to the eye, making the images of remote objects on the retinæ thereby more diffuse; so that, instinctively, the effort will either not be made or be discontinued.

The origin of absolute diverging strabismus is hereby satisfactorily explained. Unless I am mistaken, we must now, as I foresaw, invert the question as it was put above; we no longer ask—what are the concomitant circumstances which operate to bring on absolute diverging strabismus, presuming relative diverging strabismus to be already in existence? but we inquire—why it is that every relative diverging strabismus does not lead to absolute strabismus?

In the first place, I would remark, that even absolute diverging strabismus is very commonly met with (as indeed it appears every day more plainly) in cases of a high degree of myopia, much more commonly than is at all imagined. Slight degrees pass by unnoticed, because although in them the lines of vision diverge, yet the axes of the cornea show no particular divergence, sometimes even less than in non-squinting hypermetropic people. It is only when the properly directed eye is covered, that it appears that the line of vision of the other eye was directed too much outwards. I repeat, however, the question—wherefore is it that every relative diverging strabismus is not followed by absolute strabismus?

The cause thereof is partly to be attributed to the tendency to preserve binocular vision; although, in consequence of the relative diverging strabismus, the desire for the same impressions on the two yellow, and further corresponding spots, has been weakened, yet it has not been destroyed. In some cases it is this desire for binocular sight that alone resists the deviation. In many people the one eye will actually turn outwards behind the covering hand; and when this is removed, again resume its proper position; and, where the deviation does not take place, the

application of a weak prismatic glass, with the refracting angle turned towards the nose, will be sufficient to convince us of the struggle to maintain binocular vision. It is only in cases of the highest degree of myopia, in which not even well marked objects form comparable images, that, during this experiment, convergence fails to appear. From this it is manifest that sharp sight is not an absolute condition, even when the wish for single vision is clung to with the very greatest tenacity.

We look, in the next place, for the cause of the absence of absolute strabismus in limited mobility of the eyes; not only the inward, but sometimes also the outward rotation of the large ellipsoidal eyeball of myopic people is obstructed. This obstruction may go on so far that, as we have remarked above, when sharp vision, at a short distance, is required, relative converging may be joined on to relative diverging strabismus. If, however, it does not attain to this degree, it at least prevents an immoderate deviation outwards, especially where the desire to preserve binocular vision assists it.

Then, again, we find, just as in converging strabismus, various propelling and assisting forces opposed to each other; and it is, in fact, difficult to say under what conditions the former acquire the preponderance. Experience, at all events, has not, as yet, made us acquainted therewith.

Without any doubt, however, the following must be taken into account:—1. All those circumstances which facilitate motion outwards. 2. All those that lessen the importance of binocular vision. Among the former we reckon an original preponderance of the external recti; greater displacement of the lines of vision than is usual as a consequence of myopia (unusually small, or even negative value of the angle α); further, a form and superficial position of the eyeball, favourable to outward motion. Among the latter may be reckoned diminished sharpness of vision of one eye, and above all, difference of refraction in the two eyes; this last-mentioned is a factor of great importance. If the difference in refraction is great, one strong, the other emmetropic, or very slightly myopic, then it is, perhaps, the rule that in looking at distant objects the myopic eye is distorted outwards. These cases furnish a peculiar kind of diverging strabismus, which, most decidedly, deserves to be thoroughly investigated, and specially described. Sometimes, in such cases, the squinting, especially at the commencement, is intermittent, and only shows itself either when the eye is weary, or during certain mental conditions; at other times it can be overcome, although strongly developed, by an effort of the will, particularly while looking, for an instant, at near objects, but not without being speedily followed by a sense of fatigue, and the sight receives no essential advantage thereby. Not unfrequently, also, the one eye is used for looking at distant objects, the other in close vision; each eye, for the most part, projects, and judges rightly, as long as it sees

independently; and this, notwithstanding the constant assertion that the same object seen with the one eye, appears larger, and when seen by the other smaller, than it really is. There are still many curious facts to be related on this subject which merits well to be investigated. As to its pathogeny, with which alone, in fact, should we, in this place, be concerned, it is easy to perceive that, in the first place, binocular vision is not, in such cases, of much value; *secondly*, that the double images of ordinary objects (especially in seeing at a distance), are scarcely noticed, and it is consequently easy to abstract oneself from the impression of the strongly myopic eye; *thirdly*, that the limited mobility affects, in this instance, only the one eye, and a relative deviation outwards, must consequently be attended with less inconvenience; and *lastly*, that as soon as a slight exertion of the internal recti is required, in order to prevent divergence of the lines of vision, such exertion will necessarily not be forthcoming, because the slightly myopic or emmetropic eye, which sees objects distinctly at a sufficient distance, loses, in part, this faculty, in consequence of the combined effort of accommodation.

It is known that Buffon was the first to seek to determine the cause of strabismus to be a difference between the two eyes.[28] He has defined this in a manner sufficiently vague, in the following terms:—"Une inégalité de force dans les yeux." There can be little doubt but that by this he wished to express a difference in the state of their refraction; but, in his researches on strabismus, he evidently, more than once, confounds this difference with that of sharpness of sight. He attempts to demonstrate that unequal impressions of the same objects, on the corresponding points of the two retinæ, are more troublesome than images of objects altogether different (and, under certain circumstances, this may be the case), and that this is the reason why one of the eyes instinctively deviates. Buffon, in so writing, had in view, particularly, I might almost say exclusively, converging strabismus; but, towards the close of his memoir, he speaks of other cases, "where one eye was employed to look at distant, and the other at near objects, and where the one that remained unemployed, deviated, sometimes inwards, sometimes outwards." Moreover, Buffon supposes that to whatever extent the field of accommodation of the two eyes may coincide, even when their limits are partially different, both eyes can receive sharp impressions of the same object, so that the effort of accommodation of each eye, independently of the other, can be regulated according to the distance of the object. On this error a great deal of his demonstration rests.

Joh. Müller, while he admits the fact, is not satisfied with the explanation given by Buffon. He proposes another[29] very remarkable, because he assumes, for the purpose, a disturbance between the convergence and accommodation of the eyes. We do not find in Müller's work any dis-

tinction made between presbyopia and hypermetropia; he does not even distinguish whether it is convergent or divergent strabismus which he means to explain; in this way it was not possible to consider the question properly. But we find mention of an experiment which consisted in determining convergent strabismus, by placing a concave glass before one of the eyes, and then making the person regard steadily a fixed object; an experiment which explains those exceptional cases of convergent strabismus in which the correctly directed eye is hypermetropic, whilst the deviating eye is somewhat less hypermetropic; even, perhaps, emmetropic, but originally amblyopic. If Müller had placed negative glasses before both eyes it could not have escaped him but that there was an easily caused deviation inwards, and perhaps, with his penetrating glance, he would have, on the moment, recognised the nature of hypermetropia and its relations to strabismus.

The inequality of any two eyes, with respect to their sharpness of sight, or their refraction, as a case of squint, has been too much regarded by some,[30] too little by others.[31] I think I have proved that this inequality is not the immediate cause of strabismus, but, that it may become a cause thereof, under the influence of certain determining conditions existing in the non-deviating eye.

The connexion which exists between binocular myopia and diverging strabismus, has not been passed over altogether without notice. Joh. Müller even writes of strabismus of myopic people (strabismus myopum)—"Every one knows," he says,[32] in commencing an explanation of the mode of the origin of this strabismus, "that myopic people only use one eye when they look at near objects; while the other eye, also myopic, and its axis of vision obliquely deviating and directed towards distant objects, either sees not, or very indistinctly." This answers to the form which we have called relative diverging strabismus; it had already been described by Buffon, as occurring in his own eyes. In his own case he explains it by the difference of the images perceived by the two eyes; but, in general, he places the cause in the unusual convergence of the eyes required in myopic people. "The sight is fatigued and less distinct," he says, "than when but one eye is used." To this cause Müller gives the first place; but he mentions, in addition, that the refraction is augmented by the convergence of the eyes. However in explaining afterwards why the optic axis also permanently deviates, more or less, from the normal direction, he only refers to the disease of the one eye as a consequence of the deviation. Even then he does not say that the deviation is constantly outwards, and he based his demonstrations, in a great measure, on this erroneous opinion.

Ruete also speaks of the connexion between myopia and strabismus.[33] We have already seen, that whilst, in general, progressive myopia, of a high degree, induces relative diverging strabismus with a tendency to

become absolute; yet it is only exceptionally that the convergence persists in vision at short distance; but in distant vision, it is maintained at the expense of parallelism of the lines of vision. These exceptional cases, in which the convergent strabismus, relative convergent, if one will, is complicated with progressive myopia, have been noticed by Ruete; but he did not notice the more general cases. He failed to convince himself of the presence of relative diverging strabismus, because, without doubt, he searched for it in cases of too slight a degree of myopia, where it is ordinarily wanting.

In general one is but little satisfied when one consults the more recent but abundant literature of strabismus for to discover its etiology. In fact, for a long period, diverging strabismus has been completely neglected. A distinction of the various causes, according to the different forms, is not to be found, and each time that the etiology of strabismus, in general, is considered, it is evident that it is converging strabismus alone that is meant. I have yet only to allude to the writings of von Graefe, relative to the insufficiency of the internal recti, in the numerous modifications of which the gradual transition to diverging strabismus is to be sought, and, certainly, was sought for by von Graefe, and so we read in his last work:[34] "One might define insufficiency, in general terms, as a dynamic divergent strabismus, of which the degree varies, according to the distance of the objects, and which is temporarily overcome by the effort to see objects simply." We have only to recollect that this effort, in the myopic eye, becomes more and more enfeebled, for to perceive in some sort, that absolute diverging strabismus necessarily develops itself as an effort of myopia.

So our task is summed up in the following two propositions, whose antithesis is remarkable:—

1. HYPERMETROPIA DETERMINES ACCOMMODATIVE ASTHENOPIA, WHICH IS ACTIVELY OVERCOME BY CONVERGING STRABISMUS.

2. MYOPIA LEADS TO MUSCULAR ASTHENOPIA, WHICH IS PASSIVELY ELUDED BY DIVERGING STRABISMUS.

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[1] Ametropie en hare gevolgen. Utrecht, 1860, bl. 30. Archiv für Ophthalmologie. Bd. vi. 1, S. 78 u. f.

[2] Archiv für Ophthalmologie. Bd. iii., Abth. 1, S. 308.

[3] Tijdschrift voor Geneeskunde, 1860. D. vi., bl. 657. Archiv für die Holländ Beiträge zur Natur—und Heilkunde von Donders und Berlin. Bd. ii., S. 453.

[4] Archiv für Ophthalmologie. Bd. vi., 1, S. 92.

[5] Verslagen en mededeelingen van de koninglijke Academie van weetenschappen. D. xi., bl. 159.

[6] Archiv für Ophthalmologie. Bd. i., 1, S. 110.

[7] Zur vergleichenden Physiologie des Gesichtssinnes. Leipsig, 1826. S. 230.

[8] Archiv für Ophth. Bd. i., 1, S. 435. Ibid, S. 105. Neither am I convinced by the cases which Dr. Alfred Gräfe gives in his clear and concise work (Klinische

Analyse der Motilitätsstörungen des Auges. Berlin, 1858. S. 228). I see that Arlt also considers the existence of strabismus incongruus as problematical (Die Krankheiten des Auges. Band iii., S. 320).

[9] Priestley, Geschichte der Optik, übersetzt von Klugel Leipsig, 1775, S. 408.

[10] Archiv für Physiologsche. Heilkunde, 1842, S. 590.

[11] Von Graefe's important observations on the changes in the projection of the optic axes in strabismus have given origin to numerous treatises on binocular vision, in which Müller's theory of identical points has been controverted. What has been written above is so closely connected with this subject that I cannot refrain from briefly giving my opinion on it.

It is necessary to distinguish between *the projection of the field of vision* and the projection of a point in that field (*vide* Holländische Beitrage z. d. Anatom. and Physiolog., Wissenschaften, 1848, t. 7, p. 105 and Seq.) The projection of the field of vision depends on the position of the eye, and on the direction of the line of vision which we assume to be at command. To what part of the so projected field of vision we further project a certain point is decided by the spot which its image occupies on the retina.

In myopia, the image on the retina is larger than what it would be in an emmetropic eye, the angle being the same, and this in proportion as the distance between the optic centre and the retina is greater; but, on the other hand, the retinal surface is larger, owing to its extension. In cases where these two factors compensate each other the projected images on the retina might preserve the same size; but in consequence of the disproportionately large dimensions of the posterior pole in very advanced myopia, an object seen by direct vision will be projected smaller than before the extension, *i. e.*, smaller than in a normal eye. Nevertheless, in regarding an object, its size can be correctly estimated, and its limits at once accurately defined by the finger. If the lines of vision be directed one after the other towards the outlines of an object, the change which is made in the relation between the necessary muscular contraction and the projected dimensions of the object are not betrayed by any apparent displacement. When one looks at different objects—through spectacles with concave glasses —alternately by moving the head, such an apparent displacement really takes place; and if this is not observed on moving the eyes, it is to be attributed to the fact that the disturbance of the connexion between the projected size and required motion is compensated for by the deviating direction with which we regard an object through convex or concave glasses.—From what has been said it appears that, in consequence of a displacement taking place gradually, by extension, a point of the retina is projected outwards in a direction different from its original one. Now if this direction can be changed for one and the same retinal element, so as in cases of displacement to preserve its relation with the other means of perception, then it is to be supposed that the direction in question is not a congenital one, but has arisen with the other means of perception.

In the same way, the projection of the whole field of vision may, in abnormal circumstances, be changed. In a certain equipoise of the muscles, an object looked directly at, lies straight before the eyes. If this position be changed—while we imagine the same equipoise to continue (for example in paralysis, after section of the inner or outer recti, &c.), we still project the object seen by direct vision as if it was straight before us, although it be placed laterally: therefore the *projection of the field of vision is false.* The consequence of this is, that double images arise in connexion with the other eye, which will be on the same side when the eye is deviated inwards, and crossed when it is turned outwards. If, however, the affected eye be used alternately, which is generally the case in diverging strabismus, it often learns by degrees, to find out the situation of objects, so as to distinguish properly its own impressions from those of the other eye, to project correctly, and to assign a very different position to the two

objects, which have their representation respectively in the yellow spot of each eye. The two yellow spots are, therefore, no longer projected towards the same points in space. In this case, even if we employ weak prismatic glasses, correcting only a part of the strabismus, there will exist double images on the same side as the corresponding eyes, of an object whose image falls on the outside of the yellow spot in both eyes.

That this may occur after tenotomy with incomplete effect is shown by a very instructive case reported by Dr. Alfred Gräfe (work cited, p. 236). The reverse may also occur after a long-continued or congenital convergence of one eye inwards.

Herein lies the proof that in the case of an abnormal position of the optic axes each eye can learn for itself how to project its own field of vision in the right direction. Hence it follows that a projection of both fields of vision, the one over the other, may also be acquired, as a consequence of the seeking of corresponding representations in those two portions of the retina, susceptible of the most accurate perceptions, namely, the yellow spots. By projecting their impressions, the one on the other, other portions of the retina also acquire the power of corresponding, which they can lose at any moment, as it is not dependent on a fixed anatomical cause.

[12] Ametropie en hare gevolgen. Utrecht, 1862. Bl. 45. Arch. f. Ophth. Bd. VI., I., S. 92.

[13] Klinische Beobachtungen aus der Augenheilanstalt zu Wiesbaden. Erstes Heft. Wiesbaden, 1861.

[14] Nederlansch Lancet, 2 Ser, D. III., bl. 233. 'S. Hage, 1845.

[15] Lehrbuch der Ophthalm. für Aerzte und Studirende B. II., S. 520. Braunschweig, 1854.

[16] L. c. p. 537.

[17] Annales d'Oculistique. 1855. T. xxxiii., p. 177.

[18] L. c. B. iii., S. 312.

[19] Vorgl. de Haas, Geschiedkundig onderzoek omtrent hypermetropie en hare gevolgen, 1862, p. 61.

[20] Böhm, das Schielen. Berlin, 1845. I follow here de Haas almost word for word, as he mentions that he has printed this part of his dissertation from my Lectures.

[21] Archiv. f. Ophthalm. B. iii. Abth. i., S. 277.

[22] Klinische Analyse der Motilitätsstörungen des Auges. Berlin, 1858. S. 222.

[23] L. c., B. III., S. 237.

[24] Thus we read also in von Graefe (Archiv. für Ophth., B. VIII. S. 343) :—"We have already mentioned above that myopia furnishes indeed an important, but not an absolutely preponderating contingent (to muscular asthenopia). The latter would doubtless be the case if those patients affected in a high degree by myopia did not pass much more quickly from the asthenopic condition into diverging strabismus than either hypermetropic or emmetropic ones.

[25] Archiv. für Ophth., B. viii., Abth. I., S. 83.

[26] Archiv. für Ophthalm., B. III., Abth. 1, S. 309.

[27] Still more particular inquiries are being instituted in reference to this subject—the mobility of the eyes—the maximum of convergence—the effects produced by prismatic glasses, &c., in their connexion with the axis of the cornea and the line of vision; also the mode of ascertaining the centre of motion for eyes in which the refraction differ, are being ascertained "by fresh investigations of one of our pupils, Mr. Schuerman, candidate in medicine," *added to note in French translation.*

[28] Sur la Cause du Strabisme ou des yeux louches. Mémoires de l'Académie, 1743. Histoire Naturelle, Supplement IV., p. 416. Paris, 1777.

[29] Vergleichende Physiologie des Ges., S. 228.

[30] Compare Böhm Das Schielen, l. c., and Arlt, Die Krankheiten des Auges, B. III., S. 306., u. f., Prag. 1856.

[31] Compare Reute Lehrbuch der Ophthalmologie, B. II., S. 524.

[32] L. c., S. 237.

[33] L. c., B. I., S. 226.

[34] Archiv. für Ophthalmologie, Bd. viii., Abth. 2.

On the Asymmetry of the Body[a] *of the Human Skeleton.* By DR. A. STADFELDT. Read before the Meeting of Scandinavian Naturalists, held in Stockholm, in 1863. Translated from the *Bibliothek for Læger.* 5 Række, 8 Bind. April, 1864 By WILLIAM DANIEL MOORE, M.D., Dublin, M.R.I.A.; Honorary Fellow of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen.

WHEN, some years ago, I was engaged in making a sèries of measurements of infants' heads, my attention was drawn to a want of symmetry in their two lateral portions; I found that this was scarcely ever absent, and that it was often very considerable. I was at the time unable to discover the cause of the obliquity in question; it was not due, as I stated in an essay,[b] to pressure during the birth of the head, as the asymmetry is uniform in its leading characters, independently of the direction in which the child is born, and of the position which it has occupied in the uterus. I had, moreover, met with the same asymmetry in the heads of children which I had occasion to examine in the dissection of women who had died in the commencement of labour, where, consequently, pressure could not have acted, and I finally suggested that the cause of this asymmetry must be sought at an earlier period, as I had seen it in the head of a six-months fetus.

I need not say how unsatisfactory it is to look for the cause of physical laws and their deviations in "freaks of nature." Though we must most frequently stop before we arrive at the first cause of nature's manifestations, still the human mind struggles to attain as nearly as possible to the solution of the question. It will, therefore, be understood, that the phenomenon alluded to subsequently engaged my thoughts to a great extent; and when I add, that I believe I have found the key to the explanation of this constant asymmetry in the cranium of the child, I hope I have created an interest in the matter which may attract attention for a few moments.

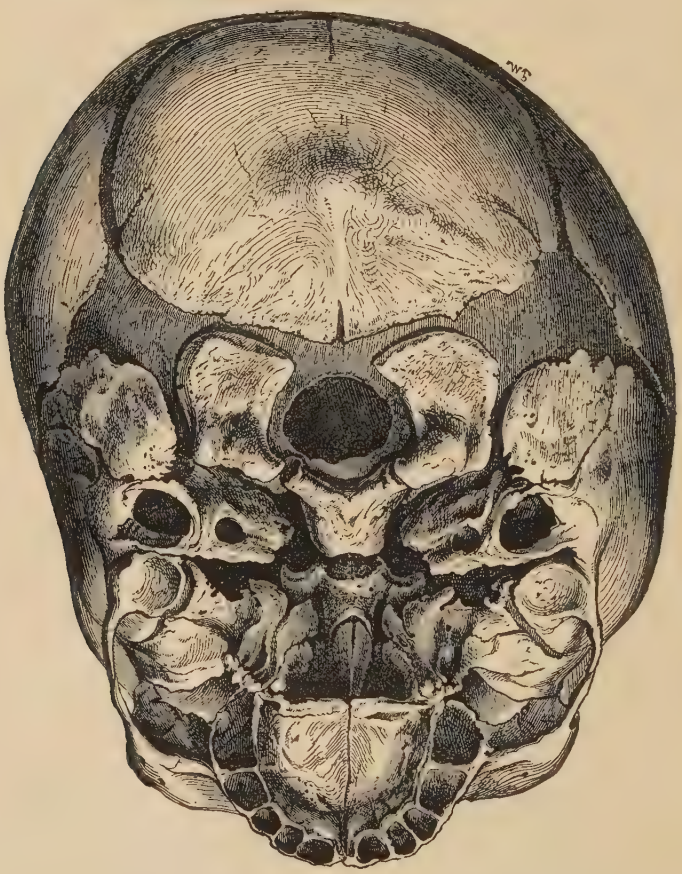
Fig. 1 represents the separated head of a child at full term, which I removed from the mother's body in dissection. After I had evacuated the brain through the foramen magnum, I immediately filled the skull carefully with gypsum, while the head rested on a cloth stretched over

[a] Including the head and trunk as distinguished from the extremities. The word, in the original is "Axedeel," or axial portion; but I have thought it better, throughout the paper, to employ the term "body," adding this note in explanation.—TRANSLATOR.

[b] Undersögelser om Barnehovedet i obstetricisk Henseende (Investigations Respecting the Infant Head in an Obstetric Point of View). Bibliothek for Læger. 5 R., 3 B., 2 H. Reviewed in the British and Foreign Medico-Chirurgical Review, July, 1862; and in the Monatsschrift für Geburtskunde, Dec., 1863.

and lying on water, to avoid pressure. This head exhibits in a well-marked degree the characters of the asymmetry referred to. In briefly describing these I would say that the two lateral portions of the head are so placed against one another that the left half is pressed upwards and backwards—the right, on the contrary, downwards and forwards.

Fig 1.



On an external inspection of the head, we have, on the one hand, the part to the left of the point of the occipital bone, and on the other hand, the squamous portion of the right temporal bone, the right ala magna of the sphenoid bone, and partly the right condyloid process, fuller and more prominent than the corresponding parts on the opposite side. This deviation from symmetry is here very well marked, and I think every one will be able to see it. It may sometimes be less striking; but, I believe that any person with an eye for form, will discover it in a greater or less

degree in all childrens' heads. I must, however, observe, that the prominence in the right temporal region is difficult to observe in the living child, as the part lies so deeply and is covered by many soft parts; at the back of the head it will, on the contrary, be easily seen when viewed from above; for it will then be found that the left half of the upper angle of the occipital and the corresponding part of the left os bregmatis are more curved and prominent than the same parts on the right side. Moreover, in the places mentioned we shall most frequently find asymmetry in the frontal portion and the arch (lacunar) itself; here it is, however, more variable, and although it is connected with the deviation pointed out in the base and lateral portions of the skull, it is, I believe, more secondary, and is more under the influence of accidental circumstances.

Let us turn to the consideration of the skull in the adult, where, as is well known, asymmetry is very general—so general, that a distinguished writer on craniology (Lucæ) has said, "that a head is scarcely ever met with which can be called mathematically symmetrical." This is, indeed, the case; but, in addition to the asymmetry which may be produced during life, by a premature synostosis in a particular suture, by unilateral muscular activity, or by morbid states of the brain and parts of the skull, it is most frequently possible to demonstrate a constant asymmetry, which has just the same characters as that described in the head of the new-born infant. We shall, in fact, find a greater fulness and prominence of the middle portion on the right side of the base and sides of the skull, and prominence on the left side of the occiput.

To understand the nature and origin of this asymmetry it is necessary to glance at the other parts of the body of the human skeleton, and especially at the spinal column. It will be seen that the spinal column forms, in the adult, a double sigmoid curvature, for the portions of the vertebral column which take part in the formation of the larger cavities of the body surrounded with bony parts (the thoracic and pelvic cavities), make a curve concave anteriorly, while the cervical and lumbar portions present a curve with the convexity forwards. Besides this curvation dependent on its mechanical relations, the spinal column has also, long since, been observed to present a lateral curve to the right in the thoracic portion, which, as it is stated (Cohen), passes into a compensating curvature in the lumbar portion, with the convexity to the left. In addition, there is often a curvation in the sacral vertebræ in the opposite direction. The pelvis too, is said (Schweighaüser) to be asymmetrical, the oblique diameter from the right sacro-sciatic notch to the left ilio-pectineal eminence being some lines greater than that from the left sacro-sciatic notch to the right ilio-pectineal eminence. These old, and in part very old, observations are perfectly correct. With reference to the spinal column it is, therefore, very rare to find a body in which we cannot

discover traces of this, we might almost say, normal scoliosis, especially when we direct our attention to the anterior surface of the spinal column; there we shall, in the dorsal vertebræ, rarely miss a curve to the right, with a frequently less evident rotation to the same side depending upon flattening of the vertebræ; in the lowest dorsal and in the lumbar vertebræ, on the contrary, we shall find a more lengthened curve to the left side, with a corresponding rotation of the anterior surface of the bodies of the vertebræ; and lastly, in the sacrum a rotation in the opposite direction. With this scoliosis in the sacrum the left ala penetrates farther into the pelvic space, the left sacro-sciatic notch is less deep, and, according to my experience, we must, in this fact, seek the cause of the diminution of the left oblique diameter of the upper outlet of the pelvis so correctly stated by the earlier writers. This report of asymmetry in the pelvis is, at least, so far correct, as in measurements I have most frequently met with the difference mentioned between the magnitudes of the oblique diameters, and only very rarely the opposite relation.

If we now refer the bones of the skull to the laws for the formation of vertebræ (Oken), and assume them to consist of three vertebræ, we shall be necessarily led to consider the asymmetry I have described as a scoliosis, the longitudinal axis being curved around the base of the skull. The scoliosis is not, it is true, so easily demonstrated here as in the spinal column, because it is spread over a greater surface; but if the eye has been made familiar with the leading characters of the asymmetry, and with the formation of vertebræ in the bones of the head, we shall find the designation correct. This is especially evident when we view the heads from below, where the posterior and middle cranial vertebræ appear so rotated that the part corresponding to the bodies of the vertebræ turns the anterior (here the inferior) surface to the left side, while the part of the posterior vertebræ corresponding to the arch (squama ossis occipitis), exhibits a curve to the same side. As it does in this head, the anterior portion of the base of the skull often also takes part in the rotation, and we then, in general, find a rotation in the opposite direction, so that the line from the middle of the lower margin of the upper jaw to the middle of the anterior circumference of the foramen magnum forms a slight curve with its convexity to the right side.

In this child's head the scoliosis is also distinct in the other parts of the cranium. Although we cannot everywhere demonstrate it by measurement, it is sufficiently evident to the eye. This is, however, far from being invariably the case with adult heads, or even in all infant heads. Only at the base and back of the skull can we always find traces of such a rotation.

There is, consequently, a normal scoliosis in the two principal parts of the body of the human skeleton, and these stand, as we shall see, in a

close relation to one another. This has, singularly enough, not been attended to, although, in my opinion, it supplies us with the only clue to the origin of the scoliosis. The connexion of the skull and the spinal column in the cervical portion is, in this respect, important; and, in fact, traces of scoliosis are seldom wanting there. Although the cervical portion of the vertebral column is too movable to admit of our assuming that the curves it exhibits in the dried skeleton have existed in the living body, even the consideration of prepared skeletons will lead our thoughts in the right direction. Thus, it is usually the case, that the two condyloid processes of the Atlas stand at different heights. This is enough to show that the axis in the connexion between the vertebral column and the head forms a curve, and not a right line. This is still further confirmed when we see that the surfaces articulating with the vertebral column at the base of the skull likewise stand in a different position both to the horizontal plane and to the vertical transverse plane through the head. From the skulls which I have examined, I have arrived at the result, that the right condyloid process of the occipital bone most frequently is both more prominent downwards towards the base, and is situated more anteriorly than the left.

Fig. 2.

This regularly occurring difference in the position of the two condyloid processes must necessarily be attended with a rotation of the cervical vertebræ, the anterior surface of the vertebræ being turned to the left side with a corresponding curve. It is, in fact, often easy to see this rotation in the bodies; and inferiorly in the two upper dorsal vertebræ the curve passes over into the opposite curve in the subjacent dorsal vertebræ.

By these considerations the connexion is established between scoliosis of the head and of the spinal column, and *the line of direction of the body of the human skeleton therefore forms an undulating line*, as in the annexed diagram.[a] I suppose that we are viewing the body of the skeleton from the inferior surface of the head, and from the anterior surface of the spinal column, and we shall then, in the great majority of cases, find an undulating line with the corresponding rotation—to the left in the two posterior cerebral vertebræ and in the cervical vertebræ; to the right in the thoracic vertebræ; to the left, from a point varying from the sixth to the ninth dorsal vertebræ, and downwards; and lastly, in the sacrum, a curve and turning of the axis to

[a] *a* Is supposed to be the anterior nasal spine of the superior maxillary bone. *b* The connexion between the head and the cervical vertebræ. *c* That between the cervical and dorsal vertebræ. *d* The transition from the dorsal to the lumbar vertebræ. *e* The sacrum.

the right side. In addition, there is often, as is indicated in the diagram, a slight inflection towards the left side of the anterior point of the line of direction; but we shall find many deviations in the last respect.

As I have shown that there is a scoliosis pervading the body of the adult skeleton, it is logical to infer that there *may* be a similar condition in the child, because the head, which participates in the scoliosis in the adult, exhibits the same asymmetry in the child as in the adult. I may also, notwithstanding statements to the contrary, assert that there is such. It is merely rather more difficult to demonstrate, because the proportions are so much less, and the change of form is, consequently, somewhat less distinct. I have examined a number of bodies of new-born infants, and in them all traces could be found of the scoliosis in question, in the upper part of the vertebral column, and especially in the thoracic portion. Even in a six-months fetus this was very evident. As to the inferior part of the vertebral column I cannot yet express a decided opinion, as the scoliosis is, in all cases, seldom so well marked there, and I must, therefore, leave it to further investigations to decide whether the scoliosis in that part is to be considered as having arisen later in life by way of compensation, or whether, as I at present believe, it also is congenital, though, as in the adult, not so well marked as in the upper part of the skeleton.[a] Besides that, I have clearly seen the scoliosis under consideration in the bodies of children. The following circumstances are in favour of its existence:—If we hang a child's body up by the feet, the face almost always turns to the one side; secondly, we find that just as the two condyloid processes in the child's head differ in their position in the same manner as in the adult, the two condyloid processes of the Atlas also stand at a different height when the head is separated from the vertebral column. I must, however, observe, that these investigations should be made previously to drying, as the latter process has a very disturbing influence on the soft and movable infant skeleton.

I now enter upon the region of hypotheses, as I am about to endeavour to assign a cause for this predominant scoliosis in the body of the human skeleton, and in doing so I am obliged to go far back into fetal life, in fact, to the oscillations of the embryo in the ovum in the first weeks of its development. It is well known that the fetus after the first weeks exhibits a rotation around its longitudinal axis, so that its central part forms an elongated spiral from the left to the right side. During a certain period of fetal life this is very well marked; subsequently it somewhat disappears, but the trace of it can still long be found; and I have met with nothing to overthrow the hypothesis, that the *scoliosis pervading*

[a] My continued examinations of the bodies of children, have led me to believe that this latter opinion is the correct one. The transition to the curve to the left begins at different points from the sixth to the ninth thoracic vertebræ.

the whole body of the skeleton is a remnant of the spiral rotation of the embryo in the ovum. On the contrary, this cause of the scoliosis in the skeleton will explain many peculiarities. By it we shall be able to understand why the scoliosis is most distinctly visible at the base of the skull and on the anterior surface of the vertebral column, because these parts, during development, are the first to acquire a firmer cartilaginous consistence, while the arches of the vertebræ, and especially the arch of the skull, have a more persistent fibrous condition, and therefore, during ossification, are more liable to modifications in their original form. Extraneous influences, the position in the uterus, pressure, &c., may thus acquire a certain importance; and we can thereby understand why these parts rarely present so pure a type of scoliosis as the infero-anterior surface. By this explanation we shall also be able to understand why the curves are most distinct in the upper part of the skeleton, for it is well known that this preponderates in the early stage of development, while the inferior part is shorter and less developed, and therefore, during growth, more easily effaces the traces of the axis-curvation.

I have here suggested a law for the line of direction in the body of the human skeleton, which may, perhaps, be modified in its several details, but the principle of which I believe to be incontestable, namely, that it forms a continuous undulating line. The transitions between the curves may vary slightly in their exact position, thus too, in the head, where, for example, the prominence on the left side of the occiput may be a little higher up or a little lower down; but a considerable deviation from the points of curvature assigned above, is very rare. We may often find skeletons where this undulating line can be demonstrated through their whole length; and there are extremely few skeletons in which we cannot, in one place or other—especially in the occiput and in the thoracic vertebræ—find traces of it; particularly when we have studied the change of form in the skeletons where it is perfectly well marked. Where, with reference to the frequency of the scoliosis in question I have spoken of "the great majority," it is, of course, understood that the numerical relation cannot be more definitely expressed; for there are, perhaps, scarcely two skeletons to be met with where the transitions between the different curves occur precisely at the same points. Moreover, we find that it would be incorrect to overlook very peculiar skeletons, or, at least, parts of such, which, indeed, exhibit an asymmetry in the two lateral halves, but where this appears not to follow accurately the general law laid down. Whether this depends merely on the fact that the turning points of the spiral are removed to other situations, or whether a rotation in the body of the skeleton deviating from the normal is possible, time will show. However, this is not an argument against the cause assigned by me, as it has already been long known to physiologists (Erdl) that a disproportion between the length of the fetus and

the size of the membranes of the ovum, or, perhaps, other circumstances, may give rise to unusual turnings of the young human embryo.

The law here propounded is not of purely scientific interest, but is also of great practical importance, because it determines many physiological and pathological deviations from symmetry in the body of the human skeleton. Thus I might direct attention to the nose which is most frequently awry, to the asymmetry in the form of the face and chest, &c., as these phenomena are, at least, in part, dependent on the same law. But as I neither can nor will treat of this subject fully here, I shall, in conclusion, content myself with referring to some points which exhibit the importance of the law in a practical point of view. Thus, those who speak of a change of form of the head from pressure in the womb or during birth, must overlook this constant, often very considerable, asymmetry in the infant head; those too, must lose sight of it, who refer to an asymmetrical form of the head in the diagnosis of nervous and mental diseases. The law in question is, moreover, of the greatest importance as bearing upon the so-called habitual or spontaneous scoliosis. This scoliosis occurs, as is well known, in the dorsal vertebræ, the curve being predominatingly directed to the right side; the cases where it is directed to the left being only two per cent. The most varying attempts have been made to explain this fact, but none appear to me to be satisfactory. The case is far different when we go back to the inclination acquired in fetal life to a curve towards the right side in the thoracic portion. When other favourable circumstances coincide, the congenital structure must determine the direction of the scoliosis. Lastly, I shall dwell upon a point which has especial interest for me as an obstetrician, namely, the importance of the law with reference to the form of the asymmetrical rachitic pelvis. It has long been observed that such pelves are most frequently diminished in their left half, the promontory being pushed over to that side. It has been asserted that this is always the case. This is, however, not so, as I have seen a few pelves where the reverse existed; but still it is true of almost all pelves of this kind, and the cause of this is sufficiently evident. The promontory lies, in fact, about at the transition from a superior curve and corresponding rotation of the axis to the left side, and an inferior rotation to the right. When the osseous mass is softened and pressure acts from above through the long curve to the left, the promontory must necessarily be pushed inwards into the left half of the pelvis. How far the physiological scoliosis through the body of the skeleton bears upon the other deformities in rachitis and osteomalacia, I must, for the present, leave undecided.

CLINICAL RECORDS.

Case of Emphysema occurring during the Second Stage of Labour. By Thomas Pratt, L.K. & Q.C.P.I.; Medical Officer to the Mountnorris Dispensary.

On the night of the 16th of May, 1864, about 11 o'clock, I was called upon to attend Mrs. H——, in her first confinement, she is a strong, healthy, well-looking, and well proportioned young woman, age twenty-two, and is over the middle size. When I arrived at her house, and saw her, it was about twelve o'clock; she then told me she had had slight labour pains all day. On making an examination I found the first stage of natural labour well advanced, the membranes protruding, and the os uteri fully dilated. At one o'clock the membranes ruptured, and a very large quantity of liquor amnii escaped; the labour pains then became brisk, but not very strong; they were of moderate duration, and everything seemed to be doing well and favourably until three o'clock in the morning, when she suddenly informed me that she could not see. She said, "I cannot open my eyes, and I cannot breath; I do not know what has come over me." This sort of information startled me; and on looking at her face I at once perceived it very much swollen. I found that it pitted deeply upon pressure, and crepitated audibly on pressure. Her neck was puffed up to double its natural size, and the skin covering the chest became a completely and rather tightly filled bag of air; however, labour went on, and about five o'clock she was delivered of a healthy male child. The emphysema extended over her whole body; her face could not be recognized by her mother or her attendant, and, had I had my midwifery forceps with me I would have shortened the duration of labour. After the child was born a binder was put on in the usual way, and on the evening of the 17th, a pair of stays, without bones or steel, was laced upon her, which made her feel very comfortable, and gave support to her in breathing.

On my next visit, on the 18th, I found the emphysema much reduced; she could then open her eyes about half. She lay on her back, which seemed to best agree with her feelings. On the 19th she was quite easy, the swelling and crepitation greatly diminished. Absorption went on from day to day, and on the 1st of June there was little to be seen of the emphysema, except upon the right side of the face, where it was still slightly present. And on the 10th of June, I am happy to say, she was quite as well as any healthy woman under ordinary circumstances.

The medical treatment was small doses of iodide of potassium, with slight friction over the face; but I do not know if either was of any service.

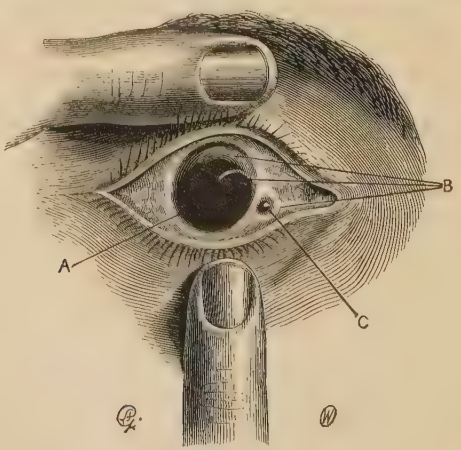
Since her recovery she had an abscess in her right breast, which had not, I think, any connexion with the emphysema.

Case of Injury of the Eye, resulting in Total Loss of Iris and Lens, with the establishment of a Permanent Fistula of the Sclerotic, nearly Perfect Vision being Recovered. By JOHN WILLIAMS, A.B., T.C.D., &c., late Surgeon to the Cork Eye Infirmary.

I must confess I hesitated some little time previous to writing the foregoing title to this case, for, although it has not been the first recorded case in which injuries of so grave a nature had been sustained by the eye-ball, still the recovery of such an amount of vision as that which the patient now possesses—notwithstanding the loss of such parts as are considered of such paramount importance to the integrity of the eye-ball as an optical instrument—not only stamps it as unique, but also invests it with surpassing interest.

I feel proud, therefore, to have been enabled to introduce the patient himself to the Cork Medical and Surgical Association,[a] and to give the Members an opportunity for careful examination of him.

Michael Haines, aged forty-six, the subject of this case, was under my care at the Eye Infirmary, for pannus, in the year 1859. I lost sight of him until the 22nd May, 1863, when he came to my residence for advice for "weakness of sight" in the right eye, the result of an injury; and for a spec on the left cornea, which, as it left the upper third of the pupil uncovered, materially interfered with, but did not completely hinder sight in that eye. On examination, which was rather cursorily made at first, the following appearances—which the subjoined woodcut, from a drawing by Mr. Humphrey Gillespie, can but imperfectly show—were presented by the right eye:—



A Clear cornea, all behind it presenting a black appearance.
B Dusky white cicatrix passing from the fistula in the sclerotic inwards to the cornea.
C Fistula in the sclerotic coat through which the vitreous humour proceeds.

[a] The subject of this most remarkable case was exhibited to the Members of the Cork Medical and Surgical Association, at their Meeting, March 23rd, 1864.

The eye-ball was misshapen, but was full, and elastic to the touch; the cornea, which was very irregular in outline, was clear and brilliant, save where it presented two whitish lines, which encroached on it as far as the centre, but which did not appear to intercept vision in the least. I must here remark that comparison with the left eye was very imperfect, as the latter presented—besides other traces of old inflammation—a leucoma on the cornea; all appeared black behind the cornea, no iris being visible; the colour of the left iris was blue, and contrasted much with the black appearance behind the right cornea. At the inferior part of the eye, towards the inner angle, and about three lines from the circumference of the cornea, was a fistulous opening in the sclerotic coat, which communicated with the interior of the globe. Through this fistula a piece of vitreous humour, about the size of a pin's head, protruded. When I made pressure with my probe on this globule of transparent humour, it receded into the depths of globe, but re-appeared when I pressed on the latter. This fistulous opening formed the apex of a conical elevation of the sclerotic, from which three or four dusky white cicatrices radiated—two, as before alluded to, through the cornea, and the other, inwardly and superiorly, through the sclerotic. The sclerotic itself, or white of the eye, was discoloured with dark blue and purplish spots, and had a few tortuous blood vessels coursing over it. There was external strabismus of that eye.

Having subjected the patient to a more careful examination with the ophthalmoscope, I could not find a trace of either the iris or the lens. A lighted candle held before the eye, and which did not appear to dazzle him in the least, caused but one erect image, showing also the absence of the lens. The orange field, presented by the back of the eye-ball and the retinal vessels, was clouded. I did not feel justified in renewing this ophthalmoscopic examination, however great my anxiety about the case may have been—as, for some days after it, he complained of unpleasant effects. It now appeared evident that the fistula, through which the vitreous humour protruded, marked the site of a rent in the sclerotic, through which the iris and lens had been bodily ejected. This rent or rupture was also evidenced by the whitish cicatrix which surrounded the fistula, and which radiated towards the inner angle of the eye, and also towards the cornea.

Notwithstanding the amount of injury sustained by the eye-ball, with the loss of such important structures, the sight of that eye is excellent, as the patient can see, *without the aid of a lens*, a single hair, pin, or other small object; and what is also most remarkable, he retains full power of adjustment of vision, for he can see the smoke issuing from a chimney 500 yards away; and by instantly directing his sight to a pin held before him, can tell what it is.

Previous history.—He had been head grinder in Perrott's Iron Foundry,

where, in the month of December, 1861, he sustained a severe injury in the right eye, from a fall on the handle of an iron plough. He suffered intense pain in the eye from the fall, and bled considerably. High inflammation set in, and the swelling of the eye-lids was so great that, for some days, he could not open the eye; when he could do so, he found he was perfectly blind in the eye, and some weeks elapsed before he "could discern day from night." Gradually, however, vision improved, and he was enabled to see his fingers, but "for a long time everything he looked at appeared red," and he was unable to distinguish the natural colour of objects until a great improvement in his sight took place. Absence from Ireland prevented my seeing him for a year after the injury.

October 3, 1863.—On this day I carefully examined him. His vision is steadily improving. He is enabled to engage in the duties of a porter, and in other occupations which do not tax his sight much.

March 22nd, 1864.—On yesterday I took the following note of this remarkable case:—I held a lighted candle before the eye, which, as I before remarked, caused no inconvenience whatever, and found I could illuminate the back of the globe—the orange field not, however, being as distinctly seen as by the light reflected from the mirror of the ophthalmoscope, the blood vessels of the retina could not be seen. When all light was excluded from the room this illumination of the globe occurred, but in a less degree. The glare of a cat's eye in the dark, or the phosphorescent appearance of fish, under the same circumstances, closely resembled the condition the eye then presented. On lighting the candle, and getting the patient to stand about six feet from it, he saw, when looking at it *without the glass*, "a great many lights," but when he approached within one foot of it he saw but one flame. I then made him use a No. 5 double convex lens, and *on looking through it*, at any distance from the candle, he saw but one flame. Although he can see a hair or pin with the *unaided* eye, still he finds the lens of much service to him when reading small print. If he look *directly* at any object, he invariably sees a "round white spot" before it, so that in order to see a thing clearly he directs his vision somewhat above, below, or to either side of it. When he uses the glass, the white spot, under any circumstances, does not appear. When he employs the glass, he holds it from a foot and a half to within a few inches of the eye, according as the object he is looking at is near or remote, large or small. He finds his sight much improved by looking through a perforated card, or piece of blackened tin, which his own ingenuity devised; but, save when he is reading, or is desirous of seeing a small object distinctly, he does not use the glass.

Remarks.—The following are the points connected with this interesting and unique case, to which I would direct special attention:—

1st. The apparent trifling deformity presented by the eye after so

serious an injury as rupture of its coats, with escape of lens, and total loss of iris also.

2nd. The wonderful amount of vision enjoyed by the patient *without the aid of a glass*, and the perfect power of adaptation or adjustment he possesses.

3rd. The fact that he is not dazzled by the brightest sun-light, and can use the eye with the same facility as if the iris were present. Although a lighted candle can be held, almost without any inconvenience whatever, immediately before his eye, still the function of the retina must be but little impaired when he is able to see so small an object as a hair without the aid of a glass.

Case of Excessive Vomiting in the Eighth Month of Pregnancy. By GEORGE H. KIDD, M.D., F.R.C.S.I.; Assist. Physician to the Coombe Lying-in-Hospital.

On the 30th of May, I was called to see a lady, residing at Sandymount, who had been suffering, for two days, from excessive vomiting. She was at the beginning of the eighth month of her third pregnancy. Her first terminated in abortion, at the end of the second month, induced, I believe, by excessive vomiting. I saw her, for the first time, at this period, and found the ovum escaping from the uterus. In the second pregnancy there was considerable sickness in the early months; but it at no time became so severe as to endanger her life, though, for a time, it caused great prostration. She went on, however, to the full period of pregnancy, and was delivered of a healthy child. In the present pregnancy there was no sickness till the end of the seventh month. When I saw her she had been sick for two days; the vomiting was almost incessant, occurring even on taking a spoonful of water, and very frequently without even this to provoke it; large quantities of green acid fluid were discharged, much more in quantity than had been swallowed; the colour was the bright green fluid, like chopped parsley, that has been found to be produced by blood, acted on by the gastric juice. She complained of great thirst, with acidity, and burning pain at the epigastrium. The bowels were confined, and their was very great prostration. I ordered creosote in two minim doses, with a quarter of a grain of opium, every second hour; mustard sinapisms to the epigastrium, and iced soda-water and brandy to be taken in small quantities, frequently. This seemed to procure relief for a few hours, but the vomiting returned with increased frequency, and nausea and great prostration. Dr. Jacob, of Maryboro', happening to be in the house, kindly saw her in my absence; he ordered solution of magnesia and aromatic spirits of ammonia, and afterwards prussic acid; but still the vomiting continued, and the prostration increased.

On the 1st of June I found the vomiting still going on, the bowels

confined, the sense of acidity, and burning pain at the epigastrium, and the thirst as before; the surface cold, the features pinched, the eyes sunken, and the pulse feeble. With a view to invert or rather restore to its proper direction, the peristaltic action of the intestinal canal, I ordered an aperient, and in a form that I have often found to be retained, and to act when no other would, namely, an ounce of sulphate of magnesia dissolved in six ounces of peppermint-water, of which I directed that a tablespoonful should be taken every hour. The iced soda-water and brandy to be continued, and cold beef tea and iced milk to be given in spoonfuls frequently repeated, even though it should be vomited off again. The abdomen to be fomented with hot water and turpentine. When I saw her again in the evening I found that no relief had been obtained, the stomach having rejected everything immediately on its being swallowed. I now ordered two draughts, each containing half a drachm of solution of morphia, hoping to procure her a night's rest; but they were both rejected, and she obtained no rest, the vomiting continuing all night.

June 2nd.—The prostration was so great that she was unable to move in the bed, and her husband implored of me to induce premature labour, which I promised to do if the symptoms did not soon yield, which I still hoped they would on getting the peristaltic action directed downwards instead of upwards. I now ordered a large turpentine and castor oil enema, to be given immediately, and to be repeated till the bowels were moved; and I directed a suppository, containing half a grain of the muriate of morphia, to be administered immediately after the enema had acted. A second enema was required before the desired effect was produced; the suppository was then given, and she slept for four hours; awoke refreshed, and with a desire for food, which she was able to take and retain. From this period convalescence was uninterrupted. She became able to eat and drink, and take abundant exercise; and on July 18th was delivered of a healthy male child, and has made a perfect recovery.

This was a somewhat unusual case of the vomiting of pregnancy, from the period at which it commenced, the violence of the attack, and the rapidity with which excessive prostration set in. As a matter of course, a suspicion of some cerebral affection, or of strangulated hernia, arose, but a careful examination afforded nothing to justify it. The vomiting of pregnancy is no doubt a reflex symptom, having its origin in some uterine or ovarian irritation. Cases are recorded in which some misplacement of the fundus, or inflammation, and even ulceration of the os and cervix, or false membrane between the fetal membranes and the uterus, have been found. The ovary, too, has been found to be the cause; but in most cases no morbid appearance can be detected after death, yet it cannot be doubted that the symptoms arise from some uterine or ovarian

irritation. The symptoms are of a twofold nature; first, there is inverted peristaltic action, causing constipation and vomiting, and secondly, under the influence of perverted nervous action morbid secretions are thrown out. The indications of treatment are:—1st. To exhaust for a time the excitability of the nerves proceeding from the lower part of the abdomen, so as to prevent their carrying to the spinal cord the morbid impressions which are reflected to the stomach; and this can be done, as physiologists are well aware, by over-stimulating them as by the free application of turpentine epithems. 2nd. To restore the peristaltic action to its natural order, to be accomplished by the use of suitable aperients, given by the mouth or by enemata. 3rd. To remove the uterine or ovarian irritation either by sedatives applied directly to the parts as by suppositories; or where there is evidence of inflammation applying leeches, or caustic, or other appropriate treatment, to its seat; and, when these means fail, and the prostration is so great as to endanger the patient's life, by inducing premature labour.

Obituary Notice.

ROBERT JOHNS, A.B., M.B., L.K. & Q.C.P.I., F.R.C.S.I.,

Died 11*th May*, 1864, *aged* 49 *years.*

To the long list of the medical men of Dublin who have recently been cut off from amongst us, we have to add the name of Dr. Johns, who died on the 11th of May, after an illness of but a few days' duration. Dr. Johns was an active and energetic practitioner, and was well known to the readers of this Journal by his frequent communications to its pages. He was one of the original members of the Dublin Obstetrical Society, of which, at an early period, he was elected an honorary member, and one of the Honorary Vice-Presidents; and, on hearing of his death, the Council held a special meeting, at which they resolved, as a mark of respect for his memory, to adjourn the general meeting of the Society that had been previously announced.

STATE OF THE ARMY MEDICAL DEPARTMENT.

(*Latest Intelligence.*)

SINCE the article on this subject (at p. 163) was printed, the Indian Medical Service Bill there alluded to has, happily for the service, been rejected by the House of Commons. An article in the *Times* newspaper characterizes the bill very justly as "a curiosity of administrative perversity." "Sir C. Wood wanted good medical officers in India, whereupon he introduced a bill which would enable him to get bad officers. The Indian Service, guarded by a competitive examination, was in the position of a purchaser who wanted a certain article, and must have it of a good quality, but at the same time offered a price less than such a quality could fetch in the open market. It was an inevitable result that the Service could not get what it wanted. Upon this, the thing to do was simply to offer the fair price; the thing not to do was to give up the necessary quality, and this was what the proposed bill did."

Deputations from the British Medical Association have waited, within the last fortnight, upon the Secretary of State for War, upon His Royal Highness the Commander-in-Chief, and upon the Director-General. In plain and clear terms the claims and grievances of the Medical Military Service were laid before them. The deputations were received civilly and politely, of course; but their protestations were in vain. From Lord de Grey they heard that it was his belief that an organized agitation existed among the medical officers of the army, which it was the duty of the Government to resist—a statement which we believe to have no other ground than that our military brethren have made a justifiable appeal to the profession at large through the columns of our cotemporaries. From the Commander-in-Chief, they heard that the granting to the surgeon his rightful position, according to his rank, would be an interference with *discipline;* and from the Director-General—we blush to write it—they heard that *third class* men were sufficiently good for the requirements of the Service, and that he would consider himself at liberty to supply the wants of the army from the list of acting assistant surgeons, which, as the Deputy Inspector General pointedly states, in the letter we have already reviewed, have come in through the "back door" of the department.

Our readers will scarcely credit our statement when we advisedly state that an acting assistant-surgeon has joined the service within the last few weeks, and commenced his military career at the tender age of *sixty years;* and this gentleman is to spend his declining years in the comforts of home service, while the unlucky wretch who should occupy his place is banished, perhaps for the second or third time in his service, to imbibe malaria in the tropics.